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CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

FURTHER OBSERVATIONS ON THE MONAURAL LOCALIZATION OF SOUND.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The present paper is a sequel to one published in the May number of the REVIEW for the current year. In the latter we reported an extended series of experiments upon a man totally deaf in one ear, showing that he possessed a somewhat surprising ability to locate certain kinds of sounds, *i. e.*, complex sounds of medium tonal range, although entirely unable to locate pure tones. Without attempting to restate in detail the conclusions we felt warranted in drawing from our observations, it may suffice to say that we connected the subject's localizing capacity with the modifications of timbre which complex sounds undergo when they come to the ear from different directions. These modifications are due to the damping or reinforcing of their constituent partials by the pinna, the external meatus, the bones of the head, etc. The present report is offered as a supplement confirming the main thesis of the previous paper by investigations upon other subjects. It brings out with special clearness the variations in monaural localization which are connected with the duration of deafness. The cases afford from this point of view a somewhat striking explanation of the contradictory reports emanating from aurists with reference to the effects of unilateral deafness upon sound localization.

Like case *A* of the earlier paper, these new cases are all

instances of internal ear deafness. We shall designate them as *C*, *D*, *E* and *F*.[1] They display in a striking manner the differences between the localizing capacity of persons who have recently lost their hearing in one ear and those who have been deaf long enough to accommodate themselves to the abnormal conditions. It does not follow from this that all persons deaf in one ear necessarily develop their latent capacities of monaural localization, and it seems probable that this is less likely to occur when deafness has come on in mature life than when it exists from early childhood. All our subjects, however, have shown ability to improve somewhat rapidly under the training afforded by the experimentation, although they have, of course, been kept in entire ignorance of the correctness or incorrectness of their judgments during the course of the tests.[2] *C* is a man of thirty, who has been deaf from early childhood. The precise time and conditions of the onset of deafness cannot be verified. The subject is under the impression that the trouble followed a

[1] We regret that Dr. Politzer's paper on 'Paracusis Loci' (*Archiv für Ohrenheilkunde*, Bd. XI., p. 231), which is the only account of extended observations on monaural localization to which we have had access, does not enter into more of detail concerning the exact methods he employed and the clinical features of the cases. These were in the main apparently instances of middle ear deafness with various degrees of defect in the hearing of one or both ears. It seems improbable that in such cases habituation to the abnormal condition, in contrast to the usual binaural capacities, should have become thoroughly established, as our tests show it may after a considerable period of unilateral total deafness. Dr. Politzer appears to have used as sounds principally the ticking of a watch and whispering. Our tests show that both of these sounds can be localized with considerable accuracy monaurally, the whispering better than the ticking. But they are less easily located than many other sounds, owing to the peculiarities of their physical composition. These peculiarities of various kinds of acoustic stimuli are more fully discussed in our first paper. This statement will, however, explain our inability to compare our results more explicitly with Dr. Politzer's.

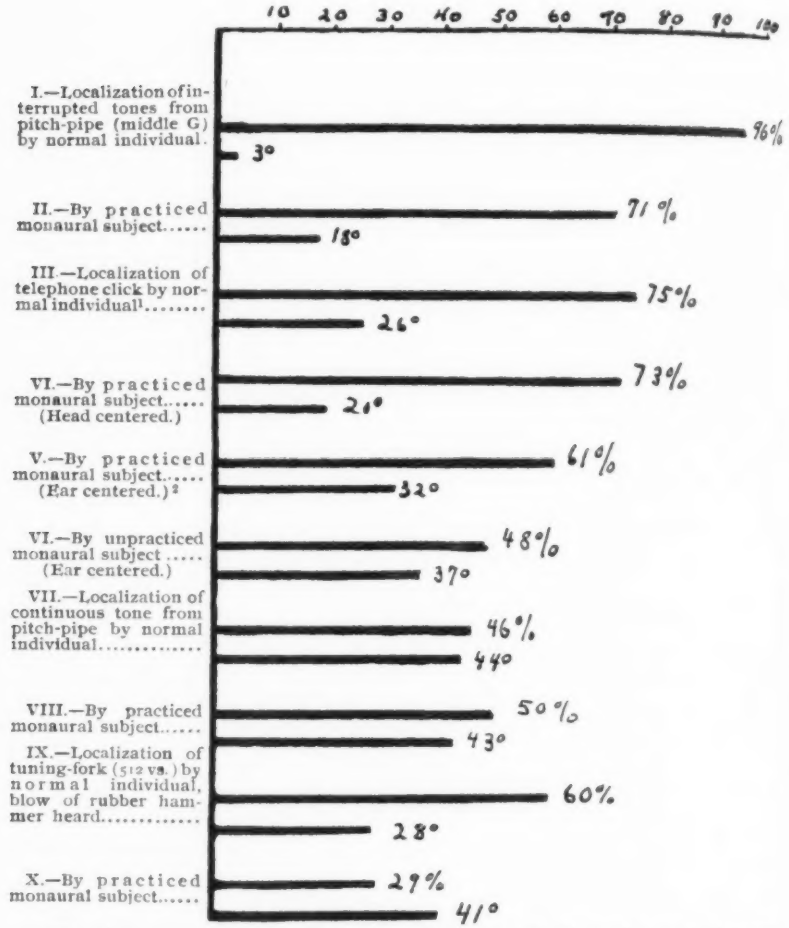
[2] The apparatus employed is described in the first paper. In the experiments recorded here the subject had the head placed at the center of the spherical cage and not the intact ear. Our observations upon *A* seemed to indicate that localization occurred with reference to the whole body and not with reference primarily to the ear. The variations due to alteration of the position in this particular are small and we have not felt warranted in taking the additional time necessary to test the matter conclusively, although the gentlemen who have served as subjects for us have been most generous in their sacrifice of time and convenience.

febrile disease. In later childhood there were transient attacks of severe vertigo. *D* is a man of twenty-four, who lost the use of one ear when ten years old, in connection with an acute attack of osteo-myelitis. *E* is twenty years old. He lost the hearing of one ear six years ago as a result of hemorrhage in the labyrinth following a severe fall. *F* is sixty years old. He was operated upon a year ago for acute suppuration of the mastoid accompanied with labyrinthine symptoms. At the time of the experiments the wound was still open. Taken in connection with *A*'s case, already reported, this gives us observations upon two persons deaf at least twenty-six years, one ten years, one six years, and one deaf for a single year.

When compared with one another the results show increased capacity of localization with increasing periods of deafness. The new cases show precisely the same relative accuracy with the different forms of auditory stimuli employed as did *A*. The more nearly a pure tone is approximated as a stimulus, the more inaccurate the localization. An improvement in the method of giving tuning-fork tones, which was also employed with *A*, resulted in securing a practically pure tone without any of the noise elements incident to setting the fork in vibration. Under these conditions all the subjects failed uniformly to localize. Occasionally such tones would seem opposite the intact ear, but quite as often no location could be assigned, and it seems quite safe to say that in monaural hearing really pure tones are unlocalizable.[1] Very high-pitched tones like those of the Galton whistle are subject to rather large errors of localiza-

[1] The earlier tests on *A*, employing tuning-forks, had been made with as perfect suppression of the composite sounds accompanying the starting of the vibrations as the means at our disposal seemed to permit. We were aware, however, that the result was not wholly satisfactory, and that we did not always secure the purity of tone desiderated. The results of the tests confirmed this in a demonstrable inability to localize most of the tones, combined with an anomalous certainty and accuracy in the localization of occasional sounds of this type. The method finally employed involves having the subject stop the intact ear with the finger until after the fork has been struck and all the overtones thus produced have died away. He is then touched with a long rod by the operator in such a manner as to convey no indication of the direction of the operator or the fork. Upon this signal he opens the ear and attempts to localize the sound. Under these conditions the localization appears impossible, regardless of the intensity of the tone.

TABLE I.



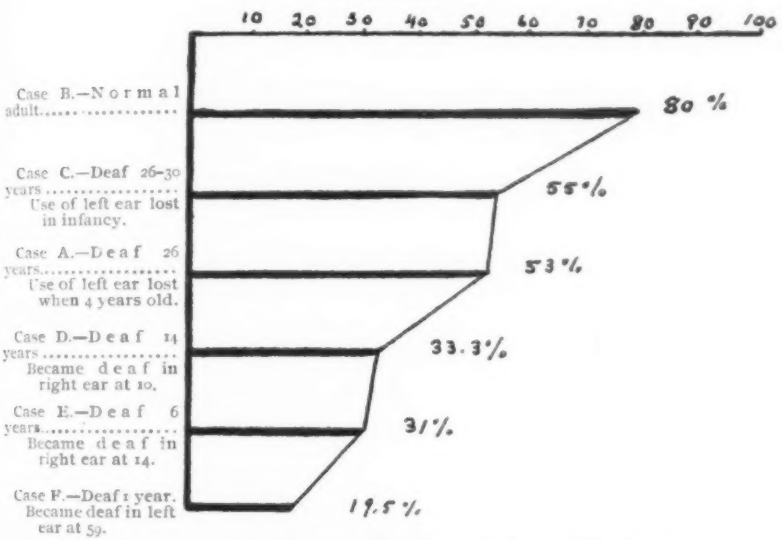
Showing comparative accuracy in auditory localization of a normal person and a practiced monaural subject. Entry VI. introduces for additional comparison an unpracticed monaural subject. Of each pair of lines the upper one represents the percentage of correct judgments, the lower one showing the average error in degrees. The tests upon which the table is based involve the localization of sixteen points (eight on the equatorial circle, four above and four below), as explained in the May number of this REVIEW (Vol. VIII., p. 232). 1,449 tests are incorporated in the table, of which approximately one-fourth were made upon normal individuals.

[1] Confusions between front and back (180°), give the average error in this case a somewhat misleading magnitude. Apart from this confusion, the errors are generally small.

[2] In this case the number showing the average error (32°), is approximate and not exact. A wayward record sheet prevents accurate verification. The possible error is small.

tion for reasons canvassed in our first paper. Complex noises like the rapid clickings of the telephone plate are localized with considerable accuracy. Similarly, complex tones near the middle of the musical scale are well localized. An ordinary reed pitch-pipe (middle G) was employed for the purpose.[1] In the case of this complex tone it proved, as in binaural hearing, considerably more difficult to effect a confident localization when the tone was continuous than when it was interrupted. Not to mention other possible reasons for this, it is certain that the identification of the tonal modifications, which are in monaural

TABLE II.



Showing by the percentage of correct judgments the variation in the capacity of monaural localization of the telephone click (eight points on the equatorial circle), as connected with the duration of deafness.

hearing the indices of direction, is much assisted by rather rapid repetitions of the tone.

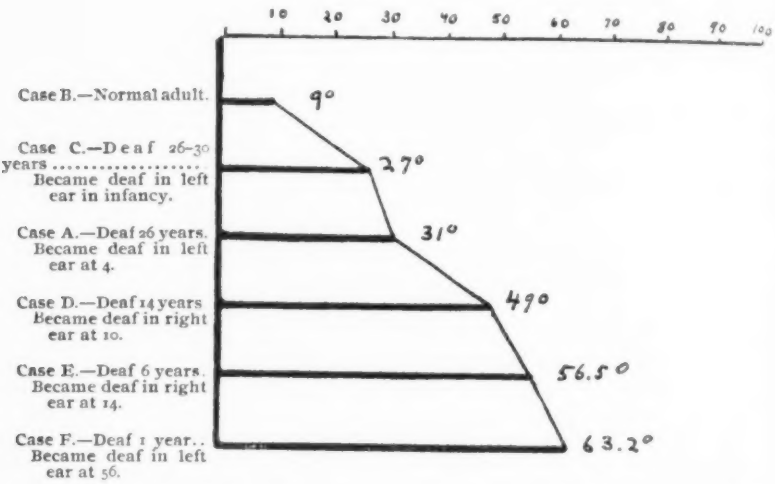
It does not seem essential to present a complete tabular statement, such as was given in the report of *A*'s case, for each form of test with the four new subjects. The differences are purely matters of degree, which can be exhibited satisfactorily in a briefer way. We have come upon no new principle, nor have

[1] This series, in the case of *A*, was not contained in the original report, but his results with this stimulus fall in perfectly with our general hypothesis.

we failed in any case to verify the hypothesis we have offered. We may first represent graphically the differences in the capacity of localization between a well-trained monaural subject like *A* and a normal person, when sounds of varying complexity are used as stimuli. Table I. exhibits these relations.

Table II. presents the variations among persons deaf in one ear, as connected with the duration of their deafness and the period at which they became deaf. The percentage of correct judgments is used as the basis of the table, the same sound, a telephone click, being used throughout the tests.

TABLE III.



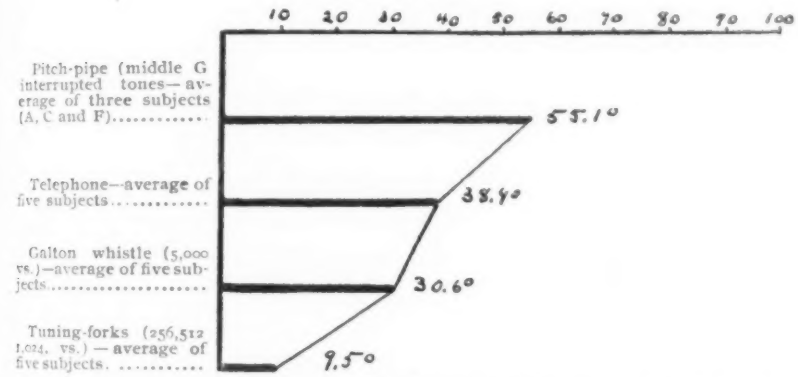
Showing in degrees the average amount of error made by the several subjects in attempting to localize eight points upon the equatorial circle. Telephone click used as stimulus.

Table III. shows the same general relations, but the average error in degrees is made the basis of the comparison, instead of the percentage of correct localizations. Table III. is probably more truly indicative of the differences between monaural and binaural hearing, and of the variations among monaural individuals themselves, than Table II.

In Table IV. we have shown graphically, on the basis of results from all the monaural cases we have examined, the variation in accuracy of localization, as compared with the noticeable

complexity in the sound. The fact should be emphasized, that all the subjects agree in their judgment of the relative ease of localization of the general kinds of stimuli, that, furthermore, their subjective impressions are verified by their objective accuracy, and that the diagram is, therefore, representative of an average, which does no violence to the relations in any individual case, as so often occurs in averages based on statistical evidence. The tuning-fork tests are included for tabular completeness only. It is perfectly certain, we think, that pure tones are unlocalizable in monaural hearing. Mere chance might be expected to produce a larger percentage of correct judgments than is here presented. But neither the percentage test nor the

TABLE IV.



Showing by percentage of correct judgments for various forms of auditory stimuli the connection between accuracy of localization and discriminable complexity of sounds (eight points on equatorial circle).

average error test is to be weighed beside the overwhelming deliverance of consciousness itself, which in the case of pure tones is absolutely different from that in the case of complex sounds.

So many of the aurists' tests are made with a watch and by means of whispering (this is true in Politzer's tests already referred to), that in view of the frequently asserted inability of monaural subjects to localize those sounds, it seems desirable to refer briefly to our own tests, which show clearly enough that such sounds, although more difficult to localize than some others, are still capable of decidedly accurate localization. The

occasional inability to localize is, therefore, probably to be connected with conditions of aural irritation, or with the lack of adaptation to the new monaural conditions.

Our tests were made in a manner already suggested in the description of the procedure in the experiments with tuning-forks. The operator, having removed his shoes, took his position at any desired distance from the subject (as a matter of fact we used various distances) and in any desired direction from him. The subject during this period kept the intact ear tightly closed. On being touched with the signal rod, he opened the ear and attempted to localize the sound. As the operator moved noiselessly, it was also possible to make tests without the stoppage of the ear. No suggestion was obtained in any case of the direction from which the sound might be expected to come, and no information was given during the series regarding the success or failure of the localization. We may quote the results of a single series of tests with each kind of sound. We quote the first tests of each kind, which represent, therefore, a minimum of familiarity with the sound and a maximum of difficulty in localization. Of twenty-two tests made by gently whispering a monosyllable like 'K' at eight different points on the equatorial circle (average distance four feet), only four mistakes occurred. They involved in no case an error of more than 45° and all four of the confusions were between points immediately opposite the deaf ear and points slightly back of that. If the confusion between front and back, which is so frequent in binaural hearing, be taken into account, this result is quite as accurate as most normal individuals would attain on a first trial. It certainly is difficult to reconcile it with any radical inability to localize.

Similarly, of the first series of tests with a watch (distance two feet) ten out of sixteen positions were correctly judged, the same points being used as in the previous test. No error was larger than 45°, and an error of this amount is frequently met with in first tests upon normal individuals, as our records show. Decidedly the watch must be used sparingly, if accurate localization is desired with either binaural or monaural persons.

In series XIII. of our earlier paper we attempted under the

limitations of indoor work to decide whether the changes in the character of complex sounds, furnishing the basis of determining their direction, were functions of changes of distance also. The indoor tests resulted negatively, the subject showing highly accurate capacity to distinguish changes of distance from those of direction, thereby demonstrating that, within the limits of variation possible under the laboratory conditions, the two series of changes need not lead to serious confusions. It seems judicious to report at this time a more drastic test made out of doors. We may anticipate the result at once, however, in saying that the test failed to show any great difficulty in distinguishing distance changes from changes of direction.

The tests were made at night, with the greatest attainable quiet, in a large open field some eight or ten acres in area. Precautions similar to those adopted in the whispering test were introduced to prevent any suggestion of the direction from which the sound might be expected. We used the same eight directions employed all through our tests, varying in entirely irregular order the distances from five to twenty-five and fifty yards. The sounds might thus come from points on any one of three concentric circles, and the subject was required to judge both the distance and the direction. As stimuli we employed the pitch-pipe previously used, and an automatic counter, resembling a pedometer in its mechanism, and producing when operated rapidly a clear, metallic, clicking sound. Indoor tests had shown that both sounds could be localized very accurately. Seventy-two tests were made, in which the direction was correctly judged 52 times, or 72 %. The average error was 14.3°, and only two errors were made in excess of 45°. These both involved the point immediately opposite the deaf ear. Of the 20 erroneous judgments, 4 also involved errors of distance. In addition to this there occurred 5 errors of distance without errors of direction. All the errors of distance were caused by confusions of sounds 25 yards distant with those 50 yards distant. When it is remembered that at the greater distances the sounds were only with difficulty audible on the side of the deaf ear, this confusion is not remarkable. On the whole, therefore, the test seems to confirm the indoor experiments in its indication that with com-

plex sounds there are relatively few confusions of distance and direction, the modifications which the sounds undergo by changes of the two kinds being differentiable in the main by the single ear. Sounds near the threshold of intensity are, as is well known, very difficult to localize in normal hearing, as well as in monaural hearing. We should expect to meet with a much larger percentage of confusions, therefore, if we employed distances so great that the sounds could barely be heard in any direction. A similar result would accrue from intrinsically feeble sounds near at hand. This is true with the watch tick, for example.

The experiments suggest very pointedly the possibility and desirability of a little training for persons who lose their hearing in one ear. If the disaster occurs in infancy or early childhood, it seems probable from our observations that experience itself serves to build up the new localizing processes. But when the injury occurs later on in life, it appears problematic how far the adaptation is carried. In any case it is well recognized that many patients suffer considerable distress through their lack of confidence in their capacity to localize sounds, and it seems probable that this distress can be largely removed by a little formal drill, by means of which the patient may have opportunity to notice carefully the changes in sounds arising from changes in their position. The most defective case we have examined (*F*) showed an increase after one hour's practice from 12.5% of correct localizations to 25%, and a decrease of his average error in degrees from 70° to 53°. This improvement occurred, too, without the subject's knowing whether his judgments were correct or incorrect. The result is attributable, therefore, merely to systematic direction of attention to the peculiarities of sounds coming from different directions. If to this were added the knowledge from visual sources of the position of the stimulus, there is every reason to anticipate a very great and rapid improvement with considerable, if not complete, relief from the distress already mentioned. The gaining of self-confidence in the matter is a large part of the victory. The discipline suggested is distinctly comparable with the ocular gymnastics prescribed to persons suffering from defects of the oculomotor mechanism, and is herewith respectfully submitted to the attention of aurists who may not employ such methods.

CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

New Apparatus.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

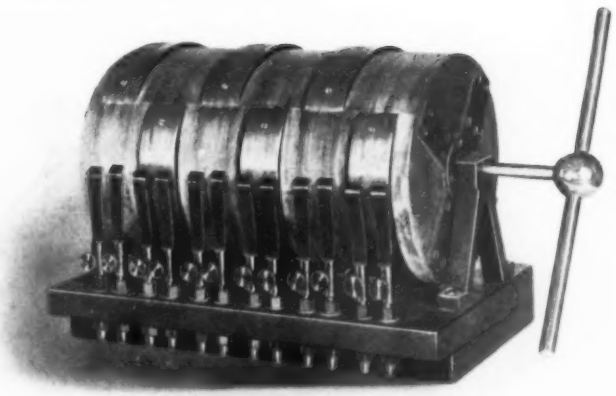
The following description is offered of several pieces of apparatus, which have been found to be of practical value in the psychological laboratory of the University of Chicago.

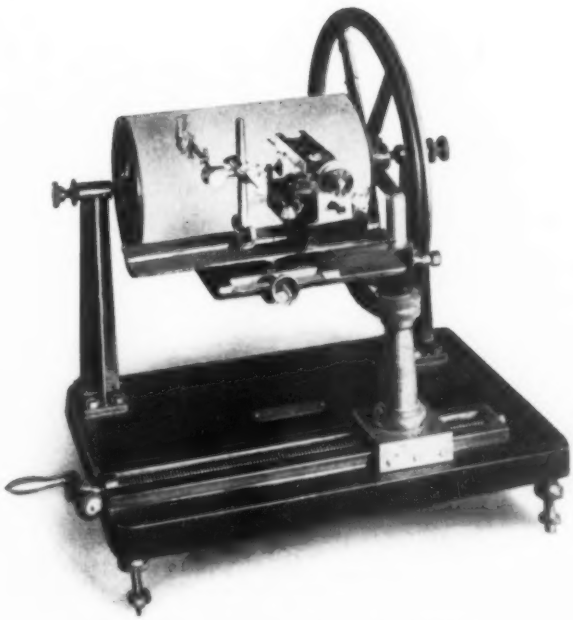
I. A multiple contact key. This is shown in Plate II. It is intended to eliminate the difficulty of adjustment and the possible source of error found in the use of the common type of bar-key. In the keys used in the Chicago laboratory it was found that the force required to ensure double contact was liable to loosen the screws by which the adjustment for simultaneity was made, also that the bar was liable to bend slightly, giving contacts which were not exactly simultaneous. The error was occasionally found to amount to 10 σ or more. No doubt these errors can be overcome by accurate workmanship. But even granting this, the extreme accuracy and the ease of adjustment of the key herewith described lead us to regard it as preferable to the bar form of key.

The key presented here consists of a cylinder of wood (length 23.5 cm., diameter 15 cm.) mounted upon steel supports, which rest upon a wooden base. Upon the surface of the cylinder are fastened six plates of brass, 25 mm. in width, and in length equal to one-half the circumference of the sphere. The thickness is 3 mm., beveled, however, to an edge at each end. They are arranged alternately along the length of the cylinder at intervals of 12 mm. where their edges rest against a straight line drawn parallel to the axis of the cylinder. Resting firmly against the cylinder are six pairs of strips of phosphor-bronze (spring-metal) which constitute the electrodes of the six

electric circuits. Each strip is 2 mm. thick, 9 mm. wide, about 8 cm. high from the platform, and curved slightly at the top, so that only the edge rests against the cylinder. The members of each pair are 6 mm. apart. Each strip is set into a circular bar of brass 25 mm. long and 9 mm. in diameter, through which there is bored a hole with a binding-screw for making connections; this bar is then continued as a screw, which passes through the projecting edge of the base and is secured to the platform by nuts above and below. This arrangement allows the strips to be raised and lowered at will. Each circuit is made when the strips rest against the brass plates of the cylinder, and broken when they rest against the wood. The key thus allows three circuits to be made and three to be broken at the same moment. Simultaneity is secured by adjusting the electrodes so that the lower edge of the pair whose circuit is to be made, rests against the upper edge of the brass plate on the line where the circuits are made and broken, and so that the upper edge of the pair whose circuit is to be broken, rests against the lower edge of its corresponding brass plate on the same line. If the electrodes were filed to a sharp edge the edges might all rest upon the same line, but this was thought to be inadvisable owing to the danger of wear and the consequent disturbance of adjustment. The width of the edge renders it also necessary to confine the adjustment to one of the two lines where the brass plates end, but this is all that the use of the key requires. The simultaneous making and breaking of the several circuits is effected by turning the cylinder, in the direction of a clock, by a rapid wrist-motion, through a semicircle; when the reaction has been made the key is turned, still in the clock-direction, back to its original position.

The key was tested on a drum, after being easily adjusted by the eye, and no error could be found. Whatever the error, it was certainly under .0001 of a second; and whatever error there may be in the adjustment, is minimized by the rapid motion of the surface of the cylinder, which, again, is due to its size. The only improvement we have to suggest is that, where the electrodes are passed through the platform, they be cut square instead of round, so as not to permit of rotation during





Angell and Fite on New Apparatus.

adjustment. Since the accuracy of the key after adjustment depends upon the stability of form of the parts, great care must be taken, in the construction of the cylinder and the base, to secure well-seasoned wood.

II. A platform for tuning-fork and marker. This is shown in Plate II., attached to a Scripture drum. It is intended to facilitate the adjustment of the several forks and markers to the drum, particularly to enable them all to be withdrawn or applied in one movement. The fork and marker rest upon a steel platform (25 cm. x 7.5 cm.) down the middle line of which is cut a groove, 20 mm. wide, with projecting flanges. The fork is secured by two screwbolts, the heads of which slide in the groove, with thumbscrews on the upper end, as shown in the photograph plate. The marker is secured to a rod 10 mm. in diameter. The rod ends below in a bolthead which slides in the groove, and is clamped to the platform by a thumbscrew. The fork and marker may thus be located anywhere along the length of the platform. This upper platform is screwed to a steel plate (100 x 80 x 5 mm.) which moves in a track to and from the drum upon a lower platform. The latter ends on the right in a socket, which fits over an upright rod (diameter 19 mm.) and thus fixes the whole apparatus to the platform of the drum. When the fork and marker have been once adjusted to the drum their relative position remains fixed. By means of the screw which appears at the front of the platform, they may, with the upper platform, be withdrawn from the drum and re-applied without any alteration of relative adjustment. The fork can thus be easily set in vibration before coming into contact with the drum. The head of the adjusting screw can easily be supplied with a circular vernier, if extreme delicacy and accuracy of adjustment are found necessary.

III. An apparatus for determining the impact limen is shown on the lower portion of Plate III. In the use of cork weights great difficulty is always experienced in placing the stimulus gently and steadily upon the skin. The instrument shown in the plate employs the principle of the balance to secure this result. It is simply a very delicate balance with three knife-edge

bearings, weighing accurately down to a milligram, mounted on a heavy steel platform raised on levelling-screws and provided with tracks at right angles to each other, upon which it travels by means of screws. In this way a considerable area of the skin can be explored without disturbance to the subject or the instrument. The mode of operation is this: The long arm at the left of the instrument carries a flat cork point, which can be detached by a screw and replaced by another of any given shape or dimensions. The cork point is attached to a flat bar sliding up into a slot in the arm. This permits a vernier arrangement, as shown on the plate. The sliding bar carrying the point is clamped firmly at any desired point by a friction-clutch, which may be seen projecting toward the body of the instrument. When the arm, supposing this to be the portion of the skin to be explored, is properly placed, the cork point is lowered, until it is just in contact with the skin, when the pointer of the balance is opposite to zero on the dial. The point is then pushed up any desired amount, say two millimeters. A weight of any desired size (for example, 10 milligrams) is placed in the pan over the arm. At the base of the support for the balance-arms are seen two levers. One of these releases the balance and permits the cork point to move gently down upon the skin. The oscillations are very slow and as the point comes into contact with the skin the balance-arm moves up again and the second lever is then pressed down, bringing the pointer to zero once more. The height of the fall and the weight in the pan can either or both be varied, and the physical expression for any given stimulus is easily computed. The instrument is noiseless and capable of rapid manipulation.[1] It must be constructed with great delicacy to secure constant action in the oscillations of the arms, and these oscillations must be slow to secure the best results. In two years and more of use it has proved very reliable. It can readily be converted into an instrument for giving passive pressure stimulations instead of impact. The principal dimensions are as follows: base plates 25 cm. square, balance arm 30 cm. long, supporting pillar and contact arm, when pushed in, each 15 cm. long.

[1] Cf. Griffing's lucid presentation of relevant considerations in his monograph, 'Sensations from Pressure and Impact,' 1896.

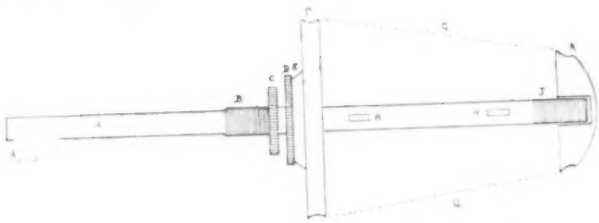
IV. A standard for adjusting a common form of the registering tambour to the surface of the drum in plethysmographic work is shown on the upper portion of Plate III. It consists of a heavy metal base carrying an adjustable upright, permitting the raising or lowering of the carriage above by means of the screw-clamp upon which, in the plate, rests the glass tube. The carriage itself is adjustable around a horizontal axis by means of a thumbscrew. It travels forward and backward by means of a track and an endless screw of fine thread. The clamp mounted upon the carriage to receive the rod of the tambour is adjustable around a vertical axis. For delicate work some such device is almost indispensable and this is as convenient as any form we have seen for tambours of this type. We give no dimensions, as these must necessarily be dependent upon the particular kymograph and tambours to be employed.

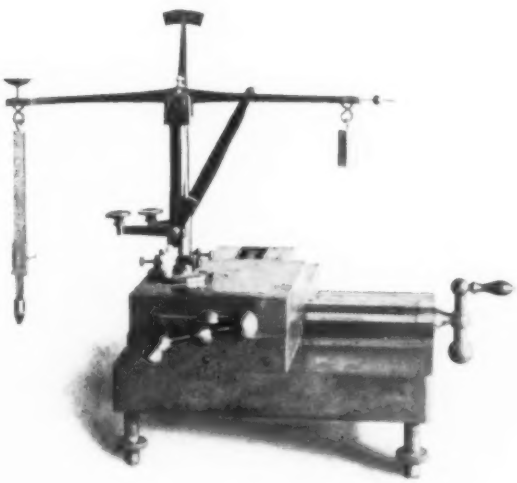
V. An adjustable form of the Hallion and Comte plethysmograph. This is shown in the sketch in the middle section of Plate III. *A* is a thin metal tube (length 15 cm.) carrying the metal disk *F* (diameter 6 cm. by 5 mm. in thickness). *A* is perforated with slits at *H* and terminates in the metal disk *K* (diameter 3 cm., thickness 5 mm.). *E* is a leather washer, 5 mm. thick, compressed and held in place by the two metal washers *C* and *D*. *G* shows the position of the rubber covering, which is tied and cemented to the grooved edges of *F* and *K*. The screw-threads at *B* and *J* on *A* permit, by changing the distance between *F* and *K*, an adjustment of the tension of the rubber covering *G*, after the joints of the rims of *F* and *K* have been made air-tight. It is thus possible to secure the maximum efficiency from the instrument without dependence upon the skill with which the rubber chances to be stretched at first. We have found the conical form of the instrument preferable to the cylindrical shape commonly employed. It permits a more perfect contact between the surface of the fingers and the rubber.

VI. Thermal apparatus (shown on Plate IV.). This consists of two parts; the first is for the purpose of maintaining a

constant temperature; the second for the purpose of locating points on the skin.

a. The first appears in the lower part of the plate, resting on the larger table. The larger and smaller vessels are for hot and cold water respectively, which passes in a continuous stream to the temperature-point on the smaller table. The problem of keeping the hot water at a constant temperature without the aid of an assistant, is solved by employing a constant flame, a constant quantity of water and a constant supply of fresh water at an approximately constant temperature. The first can usually be secured by a group of Bunsen burners; to secure the last two we used a tank of galvanized iron (diameter 35 cm., height 50 cm.), tight at the top with the exception of a hole (diameter 4 cm.) into which as a gauge we fitted a glass tube in a cork. The tank is always kept full, its fulness being indicated by the presence of water in the glass tube, which is open at the top. In order to prevent the tube from overflowing, also to avoid undue pressure within the tank, the water which comes through the feed-tube to the tank is not drawn directly from e faucet, but from a bottle with an opening near the bottom (an aspirator), which is set in a pan upon a shelf above the feed-tube, but not above the top of the tube projecting from the tank. (This part of the apparatus does not appear upon the plate.) The bottle is fed from a faucet through a tube; it is kept constantly full and the overflow is caught by the pan, from an opening at the base of which it is carried off through a rubber tube to a sink. In this manner the pressure of the water flowing through the tank is kept constant. For some purposes it is desirable to have only a small amount of water in the tank. On such occasions the pressure is of course only approximately constant. The flow of water through the apparatus as a whole is regulated by the lower glass stopcock fastened to the standard at the further end of the large table. The water flows from the temperature-point, through this stopcock, to the glass tube fixed to the standard at the rear end of the large table, thence into the funnel and out through the waste-tube to the sink. By allowing the water to fall into the funnel, instead of running out through a continuous circuit of tubing, it is possible to estimate, by the





Angell and Fite on New Apparatus.

size of the stream outlined against the black cardboard, the approximate rate of flow. The stopcock thus governs the rate of inflow as well as of outflow; if the flow outward is completely checked, the result is that all the water from the faucet overflows the aspirator into the pan and thence returns to the sink, the quantity of water in the tank remaining always the same; in this manner it is possible easily to regulate the amount of cold water coming in to cool the hot water in the tank. By adjusting the supply of gas and water any given temperature from 34° C. to 60° C. can be secured in the contact point and indefinitely retained. Much higher temperatures can readily be obtained, if necessary. The thermostatic devices employed to render such apparatus automatic can be applied, if desired. But the expense is thereby considerably increased.

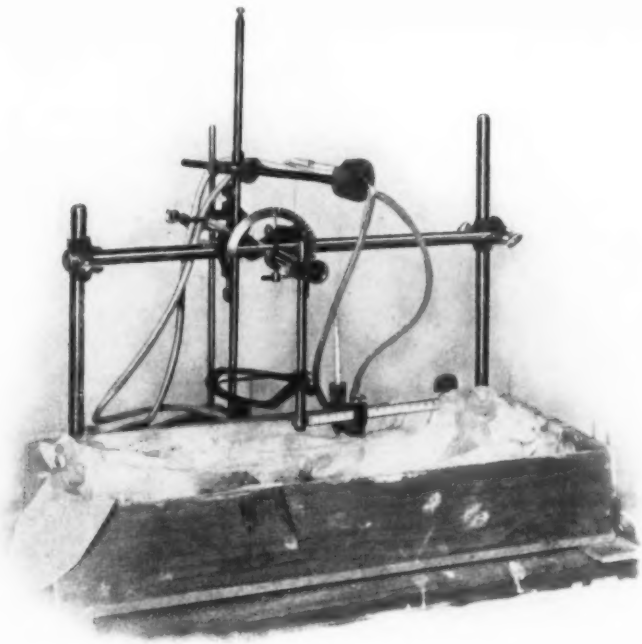
By opening and closing the proper tubes with clamps the cold-water tank is brought into the system and the hot water separated from it. The water flowing through the cold-water tank flows not directly into the tank but through a coil of lead-pipe 37 feet in length and thence out to the temperature-point. The tank itself is filled with cracked ice and salt, which cools the water during its passage through the lead-coil. Since the temperature to which the water is reduced depends upon the rate of flow, it can be regulated within certain limits by the use of the stopcock. By varying the amount of ice in the tank and, if necessary, opening the circuit from the hot-water tank, it is easily possible to secure and retain temperatures in the point ranging from + 34° C. to — 12° C. In working with temperatures below the freezing point, alcohol should be mixed with the water supplied to the coil. By substituting a small amount of ether and solid carbon dioxide for the cracked ice and salt, temperatures as low as — 100 C. can be obtained. The temperature at the copper point varies widely in this case with the length of tube, the wrapping, etc., between the point and tank.

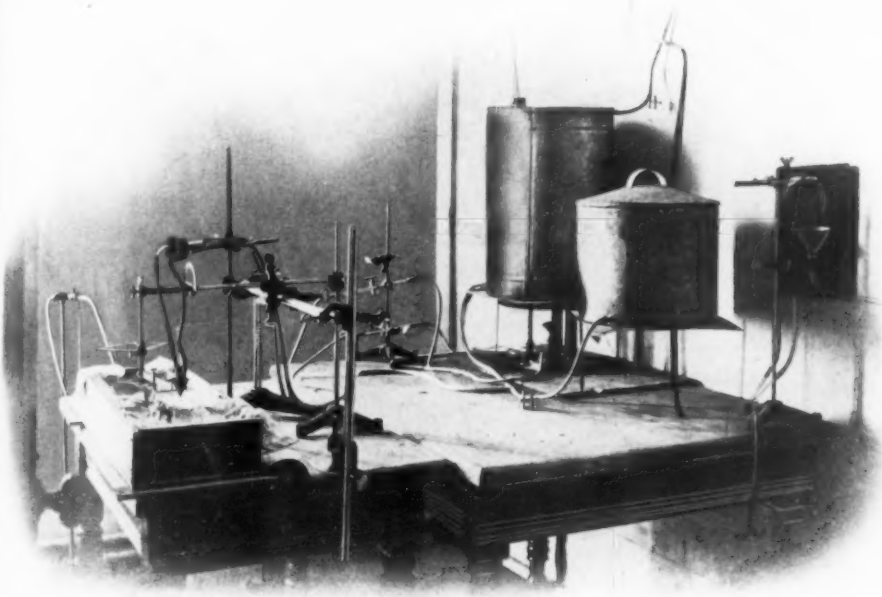
Both tanks are heavily jacketed with asbestos, which is not shown in the plate. The cut fails also to show a stopcock for drawing off the melted ice from the cold-water tank. Double tubing is used to connect the point with the cold-water tank. This has not been found necessary with the hot-water system.

In work with the extreme temperatures we cover all but the tip of the copper contact point with felt. This practically eliminates disturbing radial effects from the lateral surface of the cone. Thermally opaque screens with apertures of any desired size can of course be used, where a more complete elimination of this factor is desired.

b. The upper part of the plate shows in detail the temperature-point and its adjustments.[1] The point consists of a pyramidal vessel of thin copper, 35 mm. in height, 35 mm. in width across the top. Soldered to the top on opposite sides are two tubes (internal diameter 2 mm.); one of these extends through the top to the lower point of the vessel, which ensures the passage to that point of all the water entering the vessel. Into an opening in the top of the vessel is fixed a rubber cork, holding a thermometer, the bulb of which extends to the bottom of the vessel. For this purpose special thermometers were constructed with the particular ranges of temperature desired. The gradations show a degree to the ¼ inch. The temperature-point is fixed to the clamp which slides on the graduated rod, but is extended from it 5 cm. by means of two thin strips of spring-brass, which ensure the return of the point to its original position after being pressed against the skin. The position of the point may be varied in four ways: by rotation in a vertical plane, measured on the upper of the two circular scales; by rotation in a horizontal plane, measured on the lower circular scale; by a vertical movement of the rod holding the lower scale, regulated by the rack-and-pinion movement at the top of the rod, which acts also as a vernier; and by sliding the clamp along the lower horizontal rod. The arm upon which, in the arrangement shown by the cut, it is proposed to locate the temperature-spots, is held in a plaster cast in the large box beneath the temperature-point. The whole apparatus is clamped firmly to the table, so that, by noting the registration on the several scales, it is possible to adjust the apparatus for any desired point. If the arm remains absolutely fixed, this means an absolute location of any point on the arm.

[1] This is similar in many ways to that described by Miss Washburn in the *American Journal of Psychology*, VI, 423.





Angell and Fite on New Apparatus.

So far as we are aware, this apparatus can be employed in connection with any of the supplementary devices for securing accuracy in the localization and identification of points on the skin. It pretends to no special novelty, being merely a combination of arrangements already familiar in apparatus designed for these purposes. But it has proved very convenient and it is quite inexpensive, when compared with the more elaborate instruments, such as the kinesimeter of Hall and Donaldson.

We shall be glad to furnish further information about the cost or construction of any of the pieces of apparatus reported in this paper.

FURTHER NOTES ON THE RELATION OF STIMULUS TO SENSATION IN VISUAL IMPRESSIONS.

BY PROFESSOR C. LLOYD MORGAN, F.R.S.
University College, Bristol, England.

In a previous paper (PSYCHOL. REV., May, 1900) I gave in some detail the results so far reached in an experimental investigation on the relation of stimulus to sensation in visual impressions. I attempted to show that to obtain equal increments of sensation there are required increments of stimulus in geometrical progression. This law so stated was based on experimental data, and is a generalization directly founded on them.

In the *American Journal of Psychology* for July, 1900, Mr. Max Meyer[1] pointed out that if a constant be added to each of the terms of my stimulus-series, the stimulus-sums are in geometrical progression. This is so. And a logarithmic curve is thus obtained which gives the results of experiment plus an assumption as to what may hold good beyond the limits within which the observations were conducted. I sought to avoid any such assumption and to give a generalized statement of facts actually observed.

My investigation being experimental, I stated the results in each case in arbitrary scales, assigning 0% of stimulus and sensation to my 'black,' though fully aware that it was not absolutely black, and assigning 100% of stimulus and sensation to my 'white,' 'red,' and 'dark blue,' though it is obvious that their luminosities differed widely. In a description of experimental work this appeared to me the simplest and most direct method of statement. As in practical measurements of temperature an arbitrary zero is taken as the starting point, so did I make my 'black' the arbitrary zero of my scale. I was not

[1] For a reply to his mathematical criticisms see F. R. Barrell, *American Journal of Psychology* for October, 1900, p. 135.

dealing with an absolute zero nor attempting to obtain an absolute scale; though the possibility of translating the arbitrary into the absolute, as the result of more extended experimental work, not unnaturally presented itself to my mind.

Mr. Max Meyer in his criticism further shows that my curves for 'red on black' and 'dark blue on black' may be represented as portions of my curve for 'white on black.' Obviously they can be so represented, since they are curves of the same family, constructed on the same principle, and more limited in extent. I had myself thought of presenting them in this way, but preferred to give the results on the percentage scale by means of which they had been practically determined. In the brief notes which follow, however, I shall take advantage of Mr. Meyer's suggestions as to method of presentation.

1. Realizing that the black surface-paper I used was only a very dark gray, I first addressed myself to obtaining something more nearly approaching an ideal black. I had found the black velvet which Professor Kirschmann recommends difficult to work with. The deepest black I have yet obtained is produced by coating the surface-black with 'black enamel' paint. This gives a reflecting surface. But by so arranging matters that it receives the reflection from black cloth or velvet, good results are obtained. I now work with this new arbitrary zero and am glad to find that my results more nearly accord with Delbœuf's scale.

2. I now allow for an optical illusion which perhaps slightly vitiated my previous determination. It is well known that a circular area of, say, 3 inches radius on a black disc of, say, 6 inches radius, looks bigger than a black area of 3 inches radius on a white disc. When one is shading from inner white to outer black, the position of the mid-gray does not, for similar reasons, appear at quite the same distance from the center of the disc as when one is shading from inner black to outer white.

3. Instead of viewing the reflection of the whole disc, I now use a mirror six inches long by one inch wide. This isolates a strip of the reflected disc-surface. The accuracy of shading can be better judged in such a strip; and by marks on the edge of the mirror the position in which the mid-gray should fall can be indicated.

4. I find I was misled in a way which opens up an interesting point. I took much pains to get a mid-gray ring between an inner circle of full white and an outer circle of black. Unless I am mistaken, however, the value of such a ring in terms of luminosity is *not* the same as the value of mid-gray for continuous shading. I also endeavored to check continuous shading by obtaining a series of graded rings. But I have good reasons for believing that the curve through the points determined by means of such rings is *not* the same as the curve for continuous shading, and that the method of work I used as a check led me into error. The stimulus increments in the graded disc require a larger factor than in that for continuous shading. The difference between the curves seems to be due to contrast, the effects of which have been shown by Lehmann and others to be important. I drew attention to these contrast effects in the ring-discs, and they are clearly seen in the figure I gave. But I did not realize that they had any observable influence on the sweep of the curve. I now believe that they modify it materially, shifting the stimulus value of the mid-gray through at least 5%, probably more. In ring grading higher percentages of white are required than for continuous shading. I am not, however, prepared to discuss the question further at present.[1] More experimental work is required; and for this I shall have little time till the summer. But it opens up the possibility of obtaining quantitative estimates of the effects of contrast.

5. The net result of these corrections is that I now make 12% of white stimulus as the value of mid-gray for continuous shading with my amended black.

6. The constant to be added to each term of the series of sum-values for stimulus on this curve, so as to convert it into a logarithmic curve, is 1.895. When this constant is added the stimuli are in geometrical progression. This suggests an absolute scale obeying the logarithmic law. But we are still quite ignorant (from the experimental standpoint) of the relations of stimulus to sensation beneath the threshold of our arbitrary zero.

7. If the illumination of either a disc for continuous shading, or one with graded rings, be materially reduced, the shad-

[1] Cp. *Croonian Lecture*, Proc. R. S., Sept., 1901.

ing or grading, as the case may be, is no longer satisfactory. This fact was noted for ring discs by Delbœuf; but his hypothesis of 'tension' (*Examen critique de la loi psychophysique*, p. 148) to explain the fact is unnecessary. The occurrence of the phenomenon is a necessary consequence from the nature of the curve. Since reduced illumination lessens the luminosity of the white but leaves the black relatively unaffected, the length of the curve is diminished at its upper end. But from the character of the curve a reduction of the stimulus-luminosity of full white by, say, 12% is accompanied by a reduction of its sensation value by rather less than 3%; and the new mid-point for sensation requires relatively more white stimulus than the old mid-point required.

A particular example will serve to make this point clearer. In the well-illuminated disc the mid-point for shading from black into white requires 12% of white stimulus. Let us suppose that the illumination be reduced by one half. Instead of 100% stimulus for full white on the disc we have only 50%. Now reference to the curve (Fig. 1) shows that the sensation value of 50% stimulus is 83% on the scale of sensation. The mid-point for sensation between this and black will be $\frac{83}{2}$ or 41.5%, for which the required stimulus is seen from the graph to be 8%. So that whereas the proportional amount of stimulation for mid-gray with full illumination is $\frac{12}{100}$ that for the new mid-gray with halved illumination is $\frac{8}{50}$. In other words, the amount of white in the mid-gray has to be increased by one-third, or in the proportion of 12 to 16. This necessarily follows, in principle, from the fact that diminished illumination involves a greater proportional reduction of stimulus than of sensation. Fortunately, however, for the requirements of practical observation, a reduction of the illumination by 15% only alters the stimulus value of the mid-gray in the proportion of 12 to 12.87.

8. I have carried further the experimental work on color-shading. Making use of Sir William Abney's method of measuring the luminosity of any colored surface by matching it with a gray, I have thus determined the luminosity in terms of stimulus of five colored papers. Their positions on the white-black curve are given in the accompanying diagram (Fig. 1). If the

curve be taken as approximately correct, this gives not only their stimulus values, but their corresponding *sensation values* on the arbitrary scale. Thus is afforded a means of calculating what should be the mid-point for the shading either of the color into black, or of one color into another. In the shading of red into blue, for example, we must deal with that portion of the curve

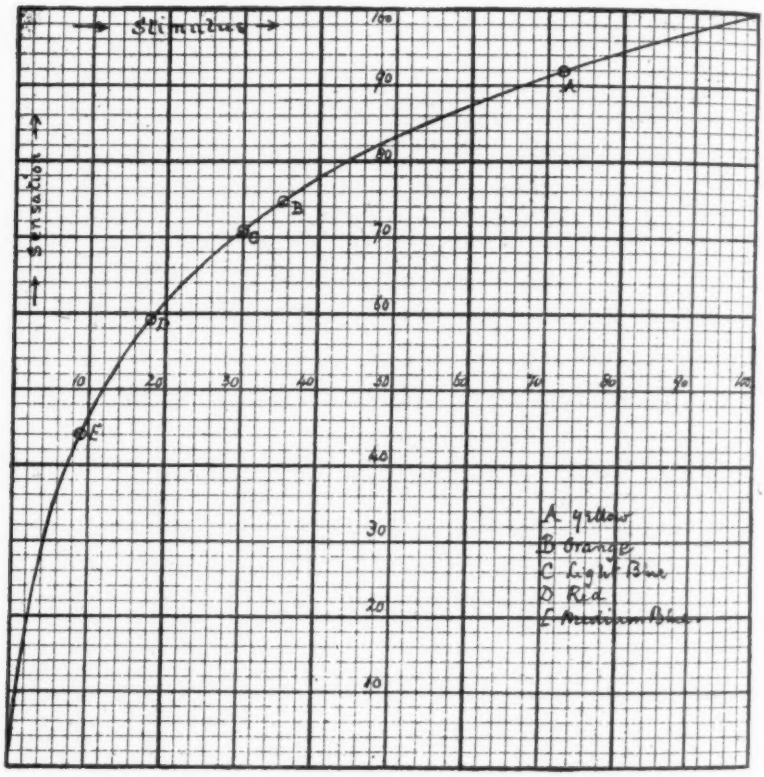


FIG. 1.

which lies within the limits assigned by the luminosity of red and that of blue respectively. In the following table the percentage mid-point is given, first as determined by experiments based on the method of shading (column I.), secondly as calculated from the curve from the luminosity as determined on Sir William Abney's method (column II.).

	I.	II.
Yellow on Black.	13.5%	13.8%
Orange on Black.	18	18.6
Light Blue on Black.	19	19.7
Red on Black.	23	23.6
Medium Blue on Black.	28	29.5
White on Medium Blue.	25	24.7
White on Red.	30	30.6
Yellow on Light Blue.	40	39.1
Red on Medium Blue.	44	43.
Orange on Medium Blue.	36	35.4

It appears from the above that the relative effect of stimulation by any colored surface is proportional to its luminosity.

9. I have redetermined the minimum visibile of white on my new black as .1 %, and that of such black on white as 1.1 %, and have made some progress in determining the minima visibilia on intermediate stages of gray. The results so far obtained seem to show that the least perceivable increments are in geometrical progression; but I am not prepared to discuss them without further experimental work. The curve obtained from them is certainly not the same as that for continuous shading.

10. I find that the minimum visibile of any color on black requires an amount inversely proportional to the luminosity of the color as compared with white. The luminosity of my red paper being 18 % (*i. e.*, equivalent to a gray with 18 % of white stimulus), the minimum visibile is by calculation $.1 \times \frac{100}{18} = .5$; the amount determined by experiment being also .5 %.

DEVELOPMENT OF VOLUNTARY CONTROL.[1]

BY J. H. BAIR.

I. INTRODUCTION.

How we acquire voluntary control over a muscle is a problem of vast interest and importance, because of the light its solution would throw upon the nature of the will; and the intense interest manifested in its solution is shown by the solicitude with which the actions of children are observed, the various experimental investigations, both direct and indirect, which are being carried on, and the many attempts to formulate theories which shall reckon and be consistent with all the facts.

This article is concerned with an investigation of the conditions and processes involved in getting voluntary control over a muscle or group of muscles. So far as we are aware, comparatively little or no work has been done by the experimental method (that here employed) toward the solution of this problem. The method usually employed is the genetic, which is at a disadvantage in two respects: (1) It is impossible to determine when a movement becomes voluntary, since the child begins its life with a series of movements. There is no uniformity of development, and any conclusions that may be drawn are necessarily too general for a definite theory of the will. (2) This method does not afford the advantage of introspection.

We have approached the problem in a definite way, selecting a muscle over which we had, as yet, no control, and by developing this control and keeping accurate records of the progress of the development and careful introspections of the accompanying mental states, discovered a number of facts, which, together with others found in the literature on the subject, justify us in drawing certain conclusions.

The muscle selected to work with was the retrahens of the

[1] No. IV. of the *Studies from the Psychological Laboratory of the University of Michigan*.

ear (see Fig. 2, p. 480). This muscle was chosen: (1) Because of its complete isolation from other muscles; (2) because of the complete inability of most persons to contract this muscle; (3) because of the comparative ease with which it can be worked, and the definite movement of the ear attending its contraction. The difficulties connected with the selection of this muscle were: (1) That of getting a record of the movement for comparison; (2) that of getting subjects unable to move their ears, and at the same time prepared to give good introspection. There were other difficulties of a more general character which will be discussed when we describe our apparatus and method of work (Section II). The above difficulties were met in a satisfactory way, as we succeeded in devising practical apparatus for recording the contractions, and also in securing desirable subjects to work with. Of the fourteen subjects who rendered us their services, only two could move their ears at the beginning of the experiment and these only when vigorously raising their brow. The remaining twelve had no idea of the movement and could not produce it, however hard they tried.

II. Apparatus and Method.

The difficulties of devising a practical apparatus for registering the ear movement on the kymograph have already been referred to. The scheme thought of and employed throughout the experiment was to have two Marey tambours connected by a rubber tube so that the lever of one tambour would respond to the movement of the other. The great difficulty was in attaching the receiving tambour to the ear. Our first method was entirely unsatisfactory and was used only until a new apparatus could be prepared. The head was placed in a kind of stocks and fastened. The receiving tambour was fastened to a standard which was brought up to the head, and the tambour was so adjusted to the ear that the lever, which had a notch in it to fit over the ear, rested on the top of the ear and communicated its movement to the kymograph. The contraction of the muscle corresponded to the downward stroke of the recording lever. The stocks were unsatisfactory for two reasons. (1) If the clamps were fastened so tightly about the head as to eliminate

all head-movements it was unendurably painful. (2) If they were not thus tightly fastened errors would come in and it was impossible to determine what part of the record represented head, and what part ear movements.

By the second apparatus these difficulties were overcome. The ear movements alone were registered. The apparatus was comfortable, and the head could be moved freely in any direction without modifying the record. Also, a tambour could be (and was) attached to each ear and a record taken of each ear at the same time without interfering with the other.

This new contrivance (helmet), a cut of which is shown below, Fig. 1, is made up of the following pieces: The first

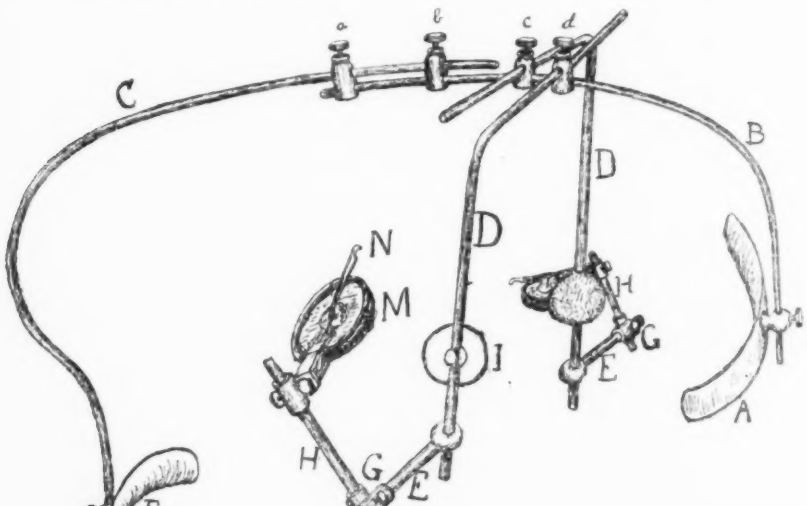


FIG. 1.

piece, *A*, is a padded plate bent to fit the forehead. It is attached by means of a post, fitting and thumbscrew to rod *B*. Rod *B* is 6 mm. thick and 25 cm. long, gently curved to fit over the median line of the head, and 6 cm. from its forward end is a sharp bend of almost 90°. At 9 cm. from the other end are two binding posts with right-angle fittings. The lower fittings are fixed to the rod *B*, the upper, through which the rod *D*

passes, are connected with thumbscrews *c* and *d*. Rods *D* are 30 cm. long and bent at right angles at their middle point. To these rods are attached the temple pads, *I*, in the same manner as *A* is fastened to *B*, and *F* to *C*. These pads, when they are properly adjusted, fit snugly to the temples. *a* and *b* are also binding posts attached to *B*, *a* at the extreme posterior end and *b* 4 cm. from it. The lower fittings are parallel with the upper, through which *C* passes. This rod *C* is 26 cm. long and is bent to fit the median line of the occipital part of the head. To its lower and posterior end is attached plate *F*, which fits the nape of the neck, is similar to *A*, and is similarly attached. The apparatus is adjusted by loosening the thumbscrews *a*, *b*, *c* and *d*. *C* and *B* are pushed together until *A* and *F* fit closely to the head, when *a* and *b* are tightened. Then *D* and *D* are pushed together until the pads *I* fit closely to the temples, when *c* and *d* are tightened. *E* is a hollow rod 9 mm. thick and 10 cm. long, with a loop at one end which fits over rod *D*. A set screw with a head the same size as the rod extends the whole length to the loop and holds *E* in the desired position. The rod *H*, of the same length and thickness as *E*, to which the tambour *M* is attached, is fastened to *E* by a right-angle clamp with set screws. *N* is the lever of the tambour and is easily adjusted to the ear so as to communicate to the drum the motion of the ear.

The other apparatus used in this experiment was that for giving the subject the idea of the movement by means of artificial contraction of the retrahens. It consisted of the following: An induction coil (alternating current), key, ammeter and electrodes. The current was kept constant by means of the ammeter and the gauge on the secondary coil. One of the wires from the secondary coil terminated in a large sponge moistened with saline solution. This was held in the hand of the subject. The other wire, also terminating in a small sponge, was periodically applied by the attendant to the retrahens of the subject; but this method was soon abandoned because there was a lack of harmony, as the attendant could not always apply the electrode at the instant the subject tried to move the ear. The electrode was now permanently adjusted to the ear and the current

was applied by the subject himself by means of a key. The electrode was held in place over the retrahens by means of a wire 28 cm. long, which was bent to fit over the top of the head. The lower end was bent downward, outward, forward, upward and inward, and held the electrode attached to it exactly in place over the retrahens.

In the second series of experiments, *i. e.*, in learning to alternate the ears, an electrode was attached to each ear in the manner just described, and the key was so arranged that the subject could stimulate first one ear and then the other.

A third series of experiments was made on a new set of subjects. Here the apparatus was the same as in the other series, except that the current was entirely dispensed with. The object of this series was to see whether the movement could be acquired without the idea of the movement first being given by means of artificial contraction.

The method followed in these experiments was uniform throughout. A record of all the experiments was kept. These were dated, numbered, and the subject's name countersigned. On this record the unusual things were noted and the probable reason stated. In a notebook were also kept the introspections of each experiment, with the date, number, and name of the subject appended.

The order generally followed in the experiment was: (1) The muscle of the ear was contracted by the current, applied about once every second, the subject at the same time remaining passive. (2) The subject tried to help the current to contract the muscle. (3) He tried to prevent the current from contracting the muscle. (4) The current was withdrawn and the subject tried to move of his own accord. The number of contractions in each of these series varied from ten to thirty. Sometimes the series were interchanged to see what difference it would make on the height of the contraction.

As to the method of the introspection, the subject was allowed to state what he had noticed in the experiment, and then usually several questions were asked, care being taken not to suggest the answer expected. Frequently the subject was requested to direct his attention in a certain way, or to notice particular

things in the course of the experiment. Each subject, before being finally discharged, was requested to state briefly the process by which he learned the voluntary control of his ears. All things were considered in making out a summary of results and in drawing conclusions.

The height of the contractions, in each series, of all the curves, was measured, and each series was averaged. The accompanying tables (I., II. and III., pp. 492, 493, 494) show the varying relations of these averages as the subjects got better voluntary control over the muscle. The curves were very easily read by means of a very thin sheet of transparent celluloid, upon which millimeter lines were cut and filled with white lead. This sheet, when placed over the record, affords a rapid and accurate means of reading it.

III. Results of the Experiment.

Before entering upon a discussion of the results of our experiment and the processes of voluntary development, we desire to call attention to Fig. 2. This sketch of the anatomy of the parts with which the experiment deals is presented in order to give the reader, who may not be familiar with them, a better idea of the arrangement of these muscles and their motor nervous connections, and also to facilitate the explanation of our facts and of the physiological processes by which a voluntary movement is acquired. The figure shows that the three ear muscles are well isolated from each other and from the other muscles of the head and face, and that the connections between them are mainly nervous. In our sketch are shown only the motor connections. The sensory nerves supplying these parts are similarly distributed and for that reason are not presented in the figure.

The retrahens (1), the muscle worked with in the experiment, notwithstanding the fact that most persons have no control over it, is adequately supplied with both motor and sensory nerves, and there is, therefore, every reason to believe that control over it is acquired in exactly the same way as control over any other muscle, and that the processes involved are identical. All of our subjects acquired control over this muscle

in the same way, and the introspections were also fairly uniform so that the results, in general, of each subject corroborate those of the others.

There were three stages involved in learning the voluntary movement of the ear. Each of these three stages will be discussed separately and a typical curve submitted to illustrate it.

FIG. 2.

Fig. 2 shows the muscles of the head and their motor nerve connections. 1. Retrahens. 2. Attollens. 3. Attrahens. The heavy lines are branches of the 7th (facial) nerve, which is the motor nerve of the head and face. The sensory distribution is not shown in this cut.

These curves, together with the tables showing the relations of these stages in heights of contractions, are given at the end of this section.

Before proceeding to a discussion of these stages it will be necessary to explain these curves. The curves presented are

ail uniform in order of series. Each curve contains four groups of contractions. These are as follows: The group marked (1) is produced by the periodic application of the current at intervals of one second. In (2) the current is similarly applied, and in addition the subject tries to help the current. In (3) he tries to prevent it from contracting the muscle, and in (4) the current is withdrawn and the subject tries to contract by voluntary effort alone. The crests and the lines between the groups of each curve represent the ear at rest. The downward and the upward lines represent respectively the contraction and relaxation of the ear.

Stage I.—This stage is represented by curve I., and is the stage before voluntary power is acquired over the muscle. As will be seen, (1), (2) and (3) are practically of equal height, except that (2) is somewhat smaller, due probably to fatigue. A definite kinæsthetic idea of the movement was given from the very start by the artificial contraction, and in every case the subject felt his ear move. All the men worked with stated that where they assisted the current (2) in contracting, their subjective experience was that the height of the contraction was increased (due to their effort), when the record showed that (2) was no higher than (1). In (3), where they tried to hold the muscle against the contracting influence of the current, none felt any power at first, but after a few experiments several felt a strong power to resist, while the others retained their sense of impotency until they really got the voluntary control. As soon as this control was acquired all held the ear contracted, as shown in (3), curves IV. and V., while at the same time they fancied they were holding the muscle relaxed in spite of the contracting current. There was some reason for believing that the subject had some power of inhibiting the contracting effect of the current even before he had acquired the power of making a voluntary contraction, as is shown by (3), curves II. and III. Curve II. is the most marked case in all our curves and was for that reason selected.

In looking over Table I., at the end of this section, *i. e.*, the table of results before the voluntary movement was acquired, it will be seen that (3) is on an average smaller than (1). This is

a fact we cannot account for. It cannot be due entirely to fatigue, for it appears when (3) is given before (1). Waller[1] made a series of experiments in which he showed the inhibition of voluntary and of electrically excited muscular contraction by peripheral excitation. His explanation, which is in terms of antagonistic muscles, or of conflicting molecules set free by the two kinds of excitation, voluntary and electrical, does not seem to satisfy our case, because there were no antagonistic muscles, and there could not yet have been any molecules set free in the muscle due to volition, as there was not yet any voluntary control. This phenomenon disappeared as soon as voluntary control was acquired, for then, as just stated, the subject unconsciously held his ear contracted.

It will be seen in (4), curve I., that although the idea of the movement was given again and again, by the contracting current, the movement could not be reproduced when the current was withdrawn. Here the idea of the movement was definitely given; but the idea of the movement does not seem to be sufficient to enable us to reproduce the movement, as it is believed to be by several prominent psychologists.[2]

[1] *Brain*, Vol. XV., p. 35.

[2] In a recent experiment on 'The Mental Life of the Monkeys,' by E. L. Thorndike, published as a monograph (PSYCHOLOGICAL REVIEW, May, 1901), the last in a series of similar experiments made on cats, dogs, chicks, etc., reported in a monograph, entitled 'Animal Intelligence' (PSYCHOLOGICAL REVIEW, 1898), it was found that monkeys, as well as cats, dogs, chicks, etc., were unable to do things from being put through them. It was seen from these experiments that the animal has not the ability to form associations except such as contain some actual motor impulse. All of Thorndike's animals failed to form such associations between the sense impressions and ideas of movements as would lead them to make the movements without having themselves beforehand in those situations given the motor impulses to the movements. These experiments show that animals do not have imitative impulses, that they do not demonstrably learn to do things from seeing or feeling themselves make the movements, much less from seeing others making them.

The question now comes, is it different with man? Can man form such an association between the sense impression and the idea of the movement that he can reproduce the movement at will? Thorndike took it for granted that he can, and this is the opinion common among psychologists. Stout has written his chapter on imitation ('Manual of Psychology') on this assumption. Baldwin has worked it out into a theory ('Child and Race'). Our experiment convinces us that man is not unlike the animal in this respect. To be put through the movement does not enable him to reproduce the movement vol-

Stage II.—This stage, represented by curves II. and III. and by Table II., is that where voluntary movement just begins to make its appearance. In this stage the retrahens is always contracted by biting the jaws together or vigorously raising the brow. The ear was thus first reached by innervating a group of muscles over which one has already control. It was reached by making it one in the group. This group is supplied by the same motor nerve (facial). One of its branches supplies the ear, as may be seen by Fig. 2. It may be questioned whether the ear movement thus effected was not due to the pulling of the skin upon it caused by the contraction of the other muscles. We are obliged to admit that part of this movement was due to these external connections. But this is not the way the voluntary control accomplished in stage III. (where the movement could be made independently of the innervation or contraction of any other muscle) was acquired, for the following reasons: (1) As soon as the brow was vigorously raised the retrahens could be seen to swell, and when the finger was placed over it the thickening could be felt every time the brow was innervated; (2) the idea of the movement had already been given by the contracting current, and all attempts to copy the movement thus produced were futile; why should not the movement caused by the pulling of the skin likewise fail? Another explanation must be sought. This will be attempted later on (section V.), after our facts have been stated and after some of the facts bearing on this problem have been reviewed.

In (4) of this stage a decided progress of voluntary control in curve III. over curve II. is shown. The innervation of the retrahens with practice and attention has become much greater and a larger movement is effected. This stage ended with a maximum contraction of the ear accompanied by a maximum contraction of all the muscles with which it was associated. There are several inferences that may be drawn from this stage of development:

untarily. However much may be said in favor of man's superior mental qualities, 'free ideas,' etc., he is nevertheless conditioned by the same laws as the animal, and cannot learn a movement apart from its chance function in a

1. Learning to contract a new muscle is a matter of association with another muscle, or group, whose voluntary movement is already known.

2. There is no sense of innervation until a movement is effected. All the subjects who did not at once succeed in making a movement stated that it seemed to them like trying to do something when they had no idea of how to accomplish it. No effort was felt until the brow was raised.

3. Learning to make a voluntary movement is largely a matter of learning to relax. Relaxation is first learned by withdrawing the attention from the movement just effected. A voluntary attempt to relax it will merely tighten its tension.

4. In learning a movement the power to contract varies with the fluctuation of attention. The fluctuations in the height of contraction at regular intervals are traceable throughout the experiment. Several long series of (3) and (4) of thirty minutes each were made; in (3), especially when fatigue began to set in, the ear was regularly relaxed at periods corresponding with the length of the attention wave, and in (4) the power to contract, when greatly fatigued, also markedly varied with the attention.

Stage III.—In this stage two things are accomplished. In the first place the maximum of contraction is reached, *i. e.*, the voluntary contraction (4) becomes larger than the contraction of the current (1), as may be seen in curve IV. This maximum was reached by concentrating, as far as possible, the motor energies upon the ear. Secondly, this maximum of ear contraction could finally be effected without raising the brow or innervating any other muscle than the one which produces the movement desired. The brow movement with which the ear was associated is gradually relaxed, until finally it is entirely eliminated and the ear can be moved independently of it. This gradual brow relaxation we are not able to show by our curves, but we can show what amounts to the same thing in curves VII., VIII., IX. and X., which are attempts at alternation of the ears, and the principle involved is the same. These curves, VII. to X., show how one ear gradually relaxes until it does not respond at all when an attempt is made to move the other.

This is the important stage in the development of voluntary

control. It begins where the ear movement has become a part of the general movement, and cannot as yet be produced except as it is accompanied by the whole group of contractions of which it is now a part. This is the stage where it is separated out and becomes independent of the group to which it had been tied. The child of a few months is practically in this stage. It has acquired a few general movements. It is now the problem of a lifetime to learn independent movements. There is no limit to the amount of analysis, of separation, of dissociation of muscles from their group, and of learning to innervate and control each individual muscle. In the general movement, so early seen in the child, all the muscles are innervated together, and all that learning to do things implies is learning to direct the impulse, at will, into this or that channel in such a way as to accomplish the end. It implies that gradually all the superfluous movements will be dropped out, and only those essential to accomplish the end will be retained. Learning to do things implies segregation, elimination, coördination, adaptation. The motor impulse, which was at first diffused throughout the group, is concentrated upon one part of the group, until that part is moved independently of the group of which it was originally a part.

Anyone who has observed a young child will have noticed how, *e. g.*, the two arms are tied together and for a long time respond together symmetrically.[1] What we have accomplished in stage II., *i. e.*, moving the ear in the group of muscles innervated by the same motor nerve, is largely given to the child phylogenetically, that is, the child is born with general movements which have been of great use to the race. These general movements Bain calls spontaneous, Preyer impulsive, and Wundt automatic. They are also often referred to by psychologists as random movements. These form the basis of voluntary movements. The child separates out of this general movement a definite movement which will help it to secure its

[1] Preyer, 'Infant Mind,' Chap. VI.; Moore, 'Mental Development of Child,' Mon. Sup., PSYCHOLOGICAL REVIEW, Part I., Sec. 2; Baldwin, 'Mental Development,' Vol. I.; Spencer, 'Psychology'; Schofield, 'Unconscious Mind of Child'; Shinn, 'Observations on Childhood,' California Studies; Preyer, 'The Senses and the Will'; Bain, 'Emotions and Will.'

freedom. First both hands respond together, then one gradually leads and the other follows, and finally, the reach is made with either hand independently of the other.[1] There is a continual breaking up of groups. One arm is moved independently of the other, then one hand, then one finger, etc. How this is done we have discovered in stage III. of our experiment, where we have the most simple possible case.

In our experiment we have had the same thing over several times, first in learning to move the ears independently of the brow, then in learning to alternate the ears, then again in learning to raise the brow without innervating the ears, and finally in raising one eye-brow independently of the other.[2]

We have already noted that prior to any attempt to alternate the ears the stimulation was applied to the left ear only. In stage I., before any voluntary ability was acquired, the curve of the right ear was a straight line. In stage II., where there was a rise of volition by association with the brow, the right ear accompanied the left except in (1) where the current alone was applied, as will be seen in the curves. This is an interesting fact and shows that the motor discharge is symmetrical, as is the case with the child with its arm movements. It took considerable practice to break up this symmetrical movement, and to move each ear independently of the other. It was much more difficult to learn than the first part of this stage, *i. e.*, learning to move the ears independently of the brow.

The introspection of this stage gave the clue to the process involved in breaking up a movement, and when fully comprehended the alternation was learned almost at once. The introspection first disclosed the way in which an independent movement is accomplished. We found that so long as we attempted to move one ear and at the same time inhibit the other, we did not succeed in alternating, but in spite of our efforts both ears responded to the same degree. But just as soon as one ear was

[1] Baldwin, 'Mental Development,' I., p. 64.

[2] It is interesting to note that before the association was made between brow and ears the brow could be raised without moving the ears. But now, ever since the ears can be moved independently of the brow, the brow cannot be raised without moving the ears. One of the subjects has since learned to raise the brow independently again.

attended to and the other was for the time being forgotten, the one attended to responded to a greater degree than the other, as is shown by curves VII. to IX. And after a little practice continuing this process the one ear could be moved to its maximum while the other remained almost entirely relaxed, as is shown by curve X.

In trying to facilitate the alternation of the ears the current was alternately applied to the two retrahens as described on page 478, and the subject tried to innervate in the rhythm of the stimulation, but here again the other ear persistently accompanied the one wished to be moved. Here the trouble was that the attention of the subject was partly directed on the inhibition of the ear not to be moved, and the victory was won only when the attention was unified. Our general conclusions for stages I., II. and III. are:

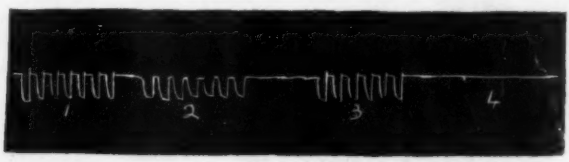
1. Before voluntary control over a muscle is acquired, it takes more than the idea of the movement of that muscle, in order to be able to reproduce the movement. The facts of stages II. and III. corroborate the conclusions of stage I.

2. We first get control over a muscle in a group, and then only can we single it out and get independent control over it. The physiological reason for this fact will be given later on.

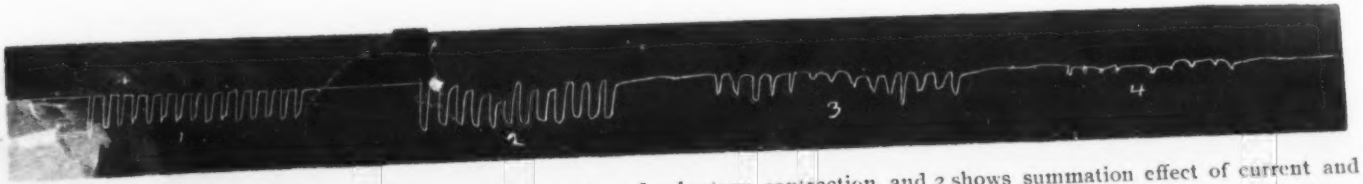
3. The more closely the attention can be directed to a movement to be made and the more nearly the part of the movement desired not to be made can, for the time being, be forgotten, the more likely is the desired movement to be accomplished.

In order to make good our conclusions a new set of subjects were engaged and the experiments were repeated, except that the current was not employed at all. These experiments were only lately begun and all of the men have not yet completed the processes. All are in the third stage. One who has been directed learned very rapidly, and can now contract his ears to the maximum without raising his brow, but has not yet succeeded in alternating. All of the rest of this set of men were left to acquire the movement in their own way. They generally were more slow to learn than the men in the first experiment. This slowness in learning may be due to the fact that they have not had the advantage of the artificial movement.

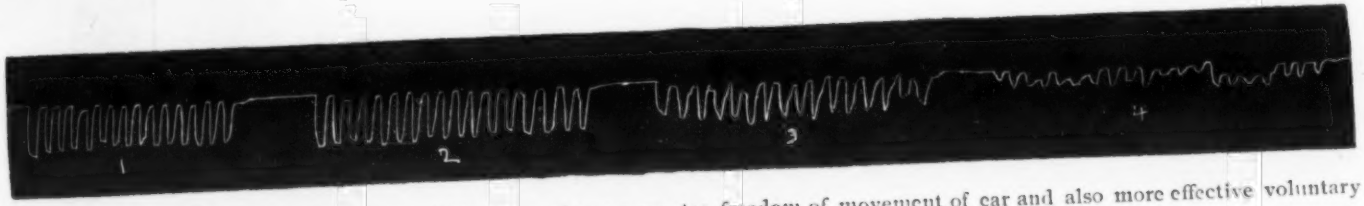
It is difficult to know what was the advantage of the artificial contraction in learning the voluntary movement. It does not seem to have opened the motor tract, over which evidently an impulse had never passed. For if it had opened the motor tract there is no reason why the voluntary movement could not be made at once. Furthermore, if the muscle had been contracted by a sensori-motor impulse, *i. e.*, if the muscle upon stimulation by the current, had been partly or wholly contracted through the sensori-motor nerve connections, only the ear stimulated would have responded when voluntary control was acquired. But this was not the case; both ears responded alike to a voluntary effort, as may be seen from curve V., notwithstanding the fact that one ear had never been stimulated. In our opinion, the sensation attending the artificial contraction furnished to consciousness the location of the part from which the sensation came with reference to other sensations contiguous to it which could be voluntarily produced. In trying to reproduce this sensation, the contiguous sensations would naturally be reproduced. And if the given sensation by an extraordinary effort should be reproduced it would at once be cognized, whereas, if it had not been given, when produced by voluntary effort it could not so easily be singled out from the group in which it is given and made the object of the attention. When a sensation has been given separately it is much more easily singled out from a group, and this would explain why the first group of men to whom the idea was given learned the voluntary movement more readily than the second group to whom it was not given. We have no conclusion from these later experiments except those which corroborate those already given.



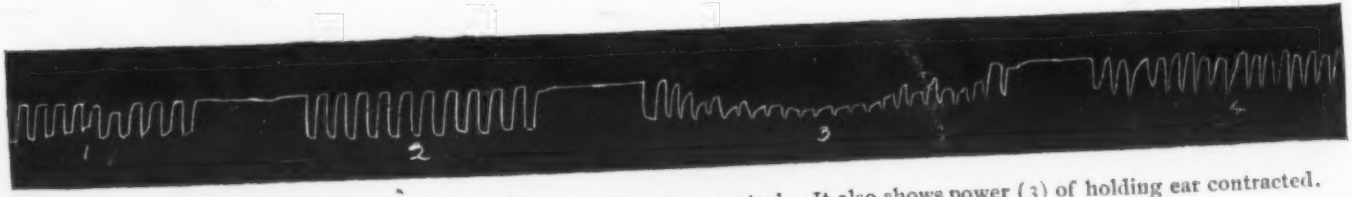
Curve I. 1 shows contraction due merely to the application of current. 2 shows contraction due to the application of the current plus voluntary effort. 3 is an attempt to inhibit the contraction produced by the application of current. 4 shows an attempt to move ear voluntarily without the help of current.



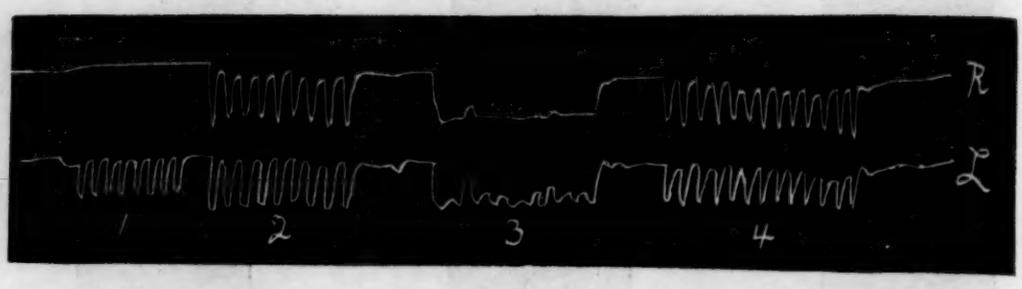
Curve II. Same as curve I. This curve shows a rise of voluntary contraction, and 2 shows summation effect of current and voluntary effort.



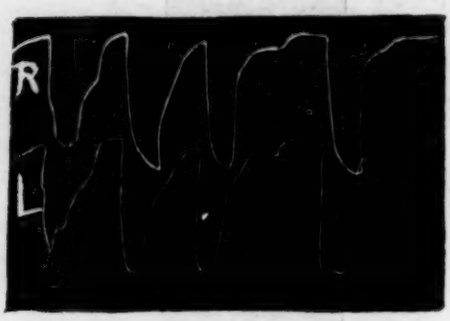
Curve III. Also same as I. and II. This curve shows a greater freedom of movement of ear and also more effective voluntary control.



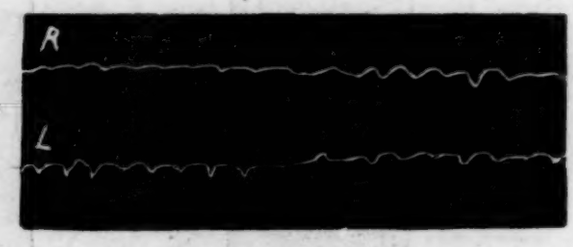
Curve IV. Same as preceding curves; shows complete voluntary control. It also shows power (3) of holding ear contracted.



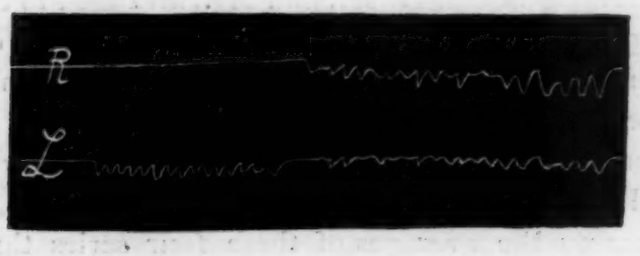
Curve V. This curve shows a simultaneous behavior of the two ears, the upper of the right and the lower of the left ear. The electrode had never been applied to the right ear, nor had there ever been an attempt to move the right ear, the current being applied only to left ear. 1 shows the response to stimulation only. The right ear in this case remains at rest. 2 shows voluntary movement plus stimulation, and it will be seen that the right ear moves as far as the left. 3 shows an attempt to hold the left ear against the movement and the right ear responds also. 4 shows voluntary contraction of the left ear and the right ear responds to the same degree.



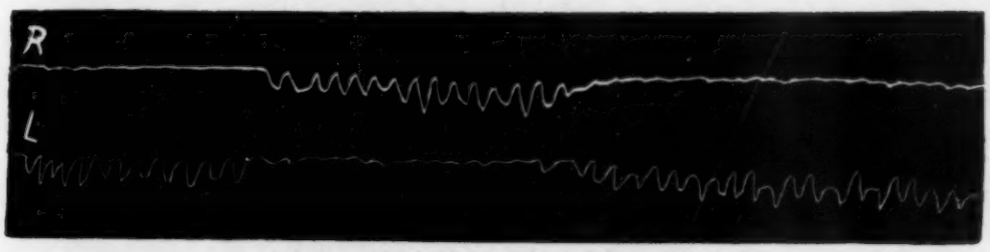
Curve VI. Shows the movement of the two ears of a subject who had no idea of ear movement when the experiment was begun, nor was the current ever applied to either ear.



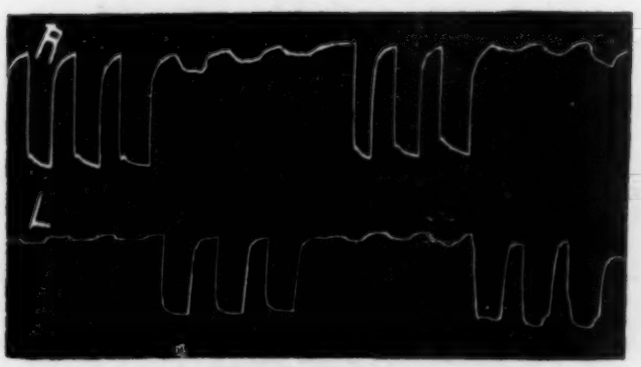
Curve VII. Shows first rise of power to hold one ear while the other is being moved voluntarily. The upper curve being the movement of right ear and lower the simultaneous behavior of left.



Curve VIII. This curve shows a more complete power to hold one ear while the other is moved voluntarily.



Curve IX. This curve shows a decided progress over curve VIII. in holding one ear relaxed while the other is contracted.



Curve X. This shows the power to contract one ear to the maximum while the other is almost entirely relaxed.

In the following tables, I., II., and III., arranged to correspond to the three stages of development of voluntary control, columns (*a*) show the average height of contraction, measured in millimeters, of each of the series (1), (2), (3) and (4) of each curve; columns (*b*) show the average of the lack of power to relax in each of the groups (2), (3), and (4), likewise measured in millimeters.

By comparing the averages of these four series throughout the tables, some idea may be formed at what rate the voluntary contraction is acquired, and also at what rate the power to relax was acquired.

Column (*b*) in (3) shows the average at which the ear was held contracted in the attempt to hold it against the current.

TABLE I.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)
Ps.	1	Nov. 2	1.8		2 1		2.0			
St.	1	" 7	1.0		.75		.75			
"	2	" 12	1.6		1.25		1.1			
"	3	" 12	1.9		1.9		2.0			
"	4	" 14	1.3		1.2		1.2			
Br.	1	" 7	1.0		.9		.95			
Cn.	1	" 8	.7		.7		.7			
"	2	" 12	1.6		.95		.8			
"	3	" 15	1.2		1.3		1.2		No Contractions.	
Ba.	1	" 1	3.0		2.8		3.0			
"	2	" 3	1.1		2.8	.4	2.0			
"	3	" 4	2.5		2.5		2.4			
"	4	" 5	2.4		2.3		2.1			
Nr.	1	" 7	.6		1.1		.7			
"	2	" 8	1.1		1.2		1.0			
"	3	" 12	1.1		1.8		2.3			
Kn.	1	" 6	1.1		1.1		1.0			
"	2	" 9	1.0		1 5		1.1			
"	3	" 19	1.4		1.8	.3	2.6			
"	4	Dec. 7	1.8		1.8		.9			
Mt.	1	Nov. 2	1.0		.8	.5	.9			

TABLE II.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)
Ps.	2	Nov. 7	1.9		2.6	.5	2.5		1.5	
"	3	" 9	1.5		1.9	.6	2.0		1.0	.4
"	4	" 12	1.6		3.0	1.4	1.9		1.5	.3
St.	5	Dec. 4	2.9		3.9	2.5	4.0	2.4	2.3	1.8
"	6	" 7	3.0		3.7	2.1	2.7	1.2	1.6	1.2
"	7	" 10	3.2		2.9	2.0	2.7	1.8	1.0	.8
"	8	" 12	2.1		2.2	1.8	2.0	1.0	.3	
"	9	" 14	3.9		4.0	2.5	3.5	2.0	.9	.5
Br.	2	Nov. 15	.9		1.0	.4	.9		.4	
"	3	" 20	2.0		2.1	.9	1.7		.5	
"	4	" 22	1.6		2.2	.5	2.0		1.5	
"	5	Dec. 4	1.4		1.4	1.2	1.1		.2	
Cn.	4	Nov. 22	2.2		2.8		2.5	1.0	.4	
"	5	Dec. 12	2.6		3.4	1.1	3.5	1.9	1.4	.5
Ba.	5	Nov. 5	3.5		5.7	1.3	2.6		.9	
"	6	" 6	2.7		3.0	.2	2.6		2.2	.5
"	7	" 8	3.4		3.6		3.3		.5	
"	8	" 9	2.5		2.9	.5	2.0		3.0	
"	9	" 9	1.9		3.1		1.5	.2	2.2	.4
"	10	" 15	2.9		3.7		2.4		1.6	
"	11	" 19	3.5		3.9	1.2	3.6	.5	1.4	
"	12	" 22	2.9		3.0		3.3	1.2	2.5	.7
Nr.	4	Dec. 11	1.2		2.4	.7	2.3	.4	.3	
"	5	" 12	2.8		2.9	1.0	2.		1.1	.6
Kn.	5	" 14	2.8		3.6	1.0	4.3	2.9	.7	
"	6	" 15	2.9		3.4	.4	2.2	1.0	1.2	.2
Ns.	1	Nov. 2	2.5		3.1	.4	1.7		2.9	
"	2	" 9	2.6		2.6	1.8	2.1	.2	2.1	
Hn.	1	" 2	2.4		2.7	.2	2.6		4.0	1.2
"	2	" 4	2.0		2.5	1.1	2.0		1.0	
"	3	" 5	1.3		3.5		1.0		1.2	
"	4	" 6	3.0		3.2	.5	2.6		2.6	.7
"	5	" 9	1.5		3.5	.4	1.8	.4	1.1	
"	6	" 9	3.2		3.2	.9	3.0		2.3	
Mt.	2	" 19	1.8		2.7	.8	1.5		1.7	
"	3	Dec. 20	L. 1.5 / R. 0		1.1	.5	.9		1.2	
					1.3	.4	.4		1.5	

TABLE III.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Ps.	6	Nov. 20	3.0		3.7	.3	2.7	1.2	1.6	
"	7	Dec. 3	2.3		2.6		2.2	.8	1.4	
"	8	" 10	3.8		4.1	.5	1.9	.7	1.8	.7
"	9	" 14	4.0		5.0		3.4	.2	3.7	1.1
"	10	" 14	3.5		4.5		2.5	1 3	2.3	
St.	10	" 13	2.6		2.1		2.6	2.0	.6	
"	11	" 14	2.0		3.3	1.8	1.6	.9	3.2	2.8
"	12	" 21	2.2		2.6		2.0	1.3	1.8	.6
Br.	6	" 4	2.1		2.1	.4	2.4	1.0	2.0	.5
"	7	" 6	2.8		3.0	.6	2 5	.7	1.5	
"	8	" 11	2.2		1.9		2.1		.8	
"	9	" 18	2.1		1.1	.2	2.0	.4	1.2	.3
Ba.	13	" 5	3.8		3.9		2 9	1.2	3.0	
"	14	" 6	3.0		3.8		2.7	1.5	3.0	
"	15	" 7	3.7		3.5		3.4	1.0	2.5	.4
"	16	" 11	3.6		3.8		2.5	1.4	2.2	
"	17	" 12	2.0		2.7		2.1	.8	1.7	
"	18	" 18	2.7		4.0	.5	2.5	1.1	4.0	
"	19	" 18	2.0		2.9		2.7	1.9	2.8	
"	20	" 18	2.5		4.2		2.5	2.1	4.4	
"	21	" 19	2.7		4.3		2.8	2.0	4.1	
"	22	" 20	4.7		5.9		5.0	2.1	5.0	
"	23	" 20	L 3.0		3.1		1.6	.7	2.7	
			R 0		1.9		1.2	1.1	1.6	
"	24	" 20	L 2.5		3.5	.3	1.0	?	3.8	
			R 0		1.5	.5			2.0	
"	25	" 21	2.7		4.0		2.6	1.5	3.7	
"	26	" 24	2.3		3.4		3.1	1.6	3.2	
"	27	" 26	5.0		6.1		4.1	.7	2.5	
"	28	" 26	L 3.0		2.7		2.7	1.8	3.0	
			R 0		2.3		2.5	1.5	3.3	
"	29	Jan. 8	L 2.5		2.7		2.3	1.6	2.3	
			R 0		1.2		1.5	.9	1.4	
"	30	" 22	4.7		5.9		5.0	3.1	5.0	
"	31	" 22	L 2.0		2.7		2.0	1.4	2.5	
			R 0		2.6		?		2.0	
"	32	" 23	L 2.4		3.0		2.0	1.4	1.8	
			R 0		2.5		?		.9	
"	33	" 28	L 3.0		4.5		4.1	3.6	3.5	
			R 0		4.8	.9	4.0	3.6	4.1	
Hn.	7	Nov. 22	3.5		3.5		3.0	.6	1.8	
"	8	Dec. 5	1.7		2.7		1.4	.9	2.9	
"	9	" 9	1.8		2.5		1.6	1.0	2.2	
"	10	" 11	2.0		2.3		1.1	.3	2.6	
"	11	" 17	2.0		4.3		1.7	.4	3.0	
"	12	" 19	2.5		4.5		2.3	.8	3.7	
"	13	" 19	2.7		4.5		2.2	1.2	4.0	
"	13	" 12	2.1		4.2		2.0	.5	4.1	

IV. Theories of the Will.

There are, roughly classifying, three general theories[1] of the will. The first one holds that voluntary movements are entirely acquired by the individual. The second maintains that the power is given by inheritance, as in the case of many animals. But in the case of man there is an after-birth evolution, and only as the organs become perfected or at least far enough developed to take up their function, do they do so spontaneously. These two theories are not consistent with the facts and need not, therefore, be further discussed. The third theory, that held by most of the prominent psychologists, physiologists and evolutionists of to-day, is a compromise between the other two. It is a conscientious attempt to be consistent with all the facts and it reckons with the data from all sources of investigation. This theory is formulated with many modifications by its different exponents. The theory was first formulated from a certain number of facts. Each time the theory was stated new facts, which were the result of observation and investigation, had to be taken into consideration, so that the theory itself underwent a gradual evolution. The theory in its latest formulation is so modified as to embrace generally the facts of evolution, child development, experiment, pathology, etc.

On account of its close relation to our facts, this theory, or rather the individual formulations of it by its several representatives, will be briefly stated. This will greatly facilitate the explanation of our facts and their bearing on these various theories.

Famous among the first modern theories that attempt to coordinate all the observed facts with the theory of evolution, and a theory which still has a rightful claim in modern psychology, is that of Bain.[2] He holds that a primordial element called spontaneous energy or surplus activity of the body, a predisposition of the moving organs to come into operation of their own accord, previous to and apart from the stimulation of the senses or of feeling, is the fundamental basis of the development of voluntary movement, that the profuse activity seen in infancy and childhood springs in a very great degree from an inherent

[1] Kirkpatrick, 'Development of Voluntary Movement,' Psy. Rev., Vol. VI.
[2] Bain, 'Emotions and Will,' pp. 304 ff.

active power, with no purpose at first except to expand itself, and that such activity gradually comes under the guidance of the feelings and purposes of the child. It is surplus energy discharging itself without waiting for the promptings of sensation, and during the course of experience and education these discharges are so linked with feelings as to be an instrument of our well-being in prompting pleasures and removing pains. Some of these random movements happen by chance to bring the organism into some happy adjustments which are pleasurable. There is an association between the pleasure and the movement which brought it about, and the memory of the pleasure, in order to prolong the gratification, incites a repetition which serves to *fix* the adjustment and make the movement more and more automatic.

Spencer's theory[1] was in many respects novel. He also starts with random movements. These are caused by a diffused discharge of molecular energy throughout the motor mechanism. There are by heredity certain paths through which the greater portion of the discharge tends to pass. But there is always a leaking from these special paths into other channels, so that every specific muscular excitement is also accompanied by some general muscular excitement. This results in diffused movements, and an adaptive movement is thus made by chance. This happy movement is 'clinched' in the following way: A successful movement is accompanied by pleasure, and consequently a large draught of nervous energy is concentrated toward the organ from whose movement the pleasure came. The previous discharge which happened to send an impulse through the line which caused the pleasurable movement has opened a channel of escape and consequently has become a line through which sufficient molecular motion is drawn to repeat the action. As the discharge is repeated the path becomes more permeable and of relatively less resistance, so that the discharge is more easily directed along this line each time, until finally the channel becomes so well defined that the discharge takes place through it without much conscious effort of the will and tends to become automatic.

[1] Spencer, 'Psychology,' I., pp. 496 ff.

Sully[1] is practically a repetition of Bain and Spencer. In brief, he holds that the child is born with certain random, reflex, and instinctive movements, which are essential to the voluntary-motor process because they bring to the child the experience of certain results of movements, *i. e.*, are pleasurable or profitable, and the association of these results with particular varieties of movement. These original involuntary movements bring about a favorable change in the child's condition, whether to lessen discomfort or to introduce a positive element of pleasure, and the child's attention is called to them. The association of the pleasure with the movement which caused it is the necessary antecedent of the conscious pursuit of an end.

James[2] also recognizes the importance of random movements as necessary antecedents of voluntary movements. He says that when a movement has once occurred in a random, reflex, or involuntary way, there remains in the memory an image of the movement which can be desired, proposed, and deliberately willed. But sensations and movements must be given before we can have a voluntary act.

Külpe[3] also holds that voluntary action is derived from accidental movements and their consequences. He accepts as far as they go the theories of Herbart, Lotze, and Bain, in their explanation of the origin of voluntary actions through the mechanical connection between the idea of the result and the movement, but if this were the only condition there could be no self-determination and man would be an automaton. Here he accepts the explanation of Wundt,[4] who gets over the difficulty by distinguishing between associative and apperceptive connections. The former are the results of given relations between ideas, while the latter imply a comparative and selective activity of the subject, and will and apperception for Wundt are but phases of the same process.

Dewey[5] likewise recognizes in the young child original motor impulses, which have no definite adjustment, but are spent

[1] Sully, 'The Human Mind,' vol. II., pp. 189 ff.
[2] James, 'Psychology,' vol. II., pp. 487 ff.
[3] Külpe, 'Outlines of Psychology,' pp. 449 f.
[4] Wundt, 'Grundzüge der physiol. Psychologie,' 3d ed., vol. II., pp. 469 ff.
[5] Dewey, 'Psychology,' pp. 359 ff.

through the whole system and give place to random movements. These original impulses are discharged through the channels of least resistance. These channels of least resistance are phylogenetically determined. The tract which has been of most use to the race is physiologically the most open, and is, therefore, the one through which the greater part of the discharge is made. Every movement is accompanied by a sensation which becomes the symbol of the movement. These muscular sensations are constantly reported to consciousness and by association we learn what act they stand for, and the movement becomes localized to the degree in which the idea of the act becomes definite. A child on first learning to do something moves the whole body, but gradually the bodily movement is eliminated, the motor impulse is more definitely directed, until finally only the part willed to be moved moves.

Preyer's theory[1] is also very similar to those already stated. It is, in short, as follows: Many reflexes of the infant at birth are already strongly marked. These have a great phylogenetic significance, because, through their frequent repetition, the harmonious co-working of many muscles as a means of warding off what might be unpleasant or injurious is soon perfected and the development of the will is made possible through these co-ordinations.

Baldwin[2] in a general way accepts the Spencer-Bain theory. He does not think, however, that the association which causes the repetition of any movement is between the pleasure of a happy chance adaptation and the movement which brought the pleasure. He asserts that the association is rather between the pleasure and the pleasure-giving thing, which is not necessarily contained in the fact of one movement rather than another. The pleasure is not in the movement but in what it gets for the organism. Another point at which he digresses is in his theory of imitation. Spencer and Bain hold that all our voluntary movements are *copies* primarily given in the random, reflex and instinctive movements, and that the individual, again to use Spencer's term, *clinches* the movements by associating them

[1] Preyer, 'The Senses and the Will,' pp. 326 ff.
[2] Baldwin, 'Mental Development, I., pp. 367 ff.

with the sensations accompanying them. Baldwin, on the other hand, insists that the adaptive movement is not necessarily such a copy, but that the new movement may be an imitation of a movement, the idea of which is given through some other than the kinæsthetic sense. Through the *try-try-again* disposition, especially of children, 'external copies,' *i. e.*, those given through sight or audition, are persistently imitated; and, to quote his own words, "The accommodation of an organism to a new stimulation is secured, apart from happy accidents, by the continued or repeated action of that stimulation, and its repetition is secured, not by the selection beforehand of this stimulation, nor by its fortuitous occurrence alone, but by the proximate reinstatement of it by a discharge of the energies of the organism, concentrated as far as may be for the excess stimulation of the organs most nearly fitted by former habit to get that stimulation again."[1]

With this brief statement of the general theories as to the rise of voluntary control of the muscles and the factors involved in a volitional act, we will proceed to the interpretation of our results and their bearing on these theories.

V. Interpretation of Facts.

In another section (III.) we have already attempted to describe the facts of this experiment. It now remains for us to interpret these facts and state their bearing on the theories above mentioned.

One of the aims of the experiment was to determine whether the definite idea of a movement is sufficient to enable one to reproduce it at will. Our results justify us in saying *No*. As has already been explained in the preceding discussion, a definite kinæsthetic idea of the movement of the undeveloped ear muscle was given by the artificial contraction of that muscle. The idea of the movement thus produced was a memory image of the sensation which was the concomitant of the movement, plus also a visual memory image of the movement. Yet the contraction could not be made voluntarily, not even after it was repeated a sufficient number of times thoroughly to impress the

[1] Baldwin, op. cit., p. 179.

sensation and definitely to fix the association between the muscular sensation and the visual impression of it. With all this preparation the subject failed to reproduce the movement voluntarily and there was no sense of innervation. To refer again to the introspection, the subjects could feel no output of nervous energy, or, to quote the words of one of the subjects, "It seemed like trying to do something when you have no idea how it is done. It seemed like willing to have the door open or some other thing to happen which is beyond your control."

All that was done by this preliminary work, *i. e.*, producing the contraction artificially, was to give a vague idea of the direction the motor impulse would have to take to reach the ear. And in the great effort to reach the ear a great deal of motor energy was discharged into muscles lying approximate. The brow was raised, the teeth clinched, and this proximity was perceived and a more strenuous effort was made to get closer, until finally the ear was budged. As soon as the ear was reached the sensation was cognized and it became the fixation point of the attention. In other words, the attention was narrowed down from the general sensation of the innervation of the adjacent muscles to the sensation of the specific movement sought for. The first time the ear movement was effected by voluntary effort the sensation was but faintly recognized, on account of the feeble discharge going to this muscle as compared with that going to the other muscles of the group. Here the subjects who had had the benefit of the artificial contraction had a positive advantage over those who had not, in that they had already a definite memory image of the movement, and as soon as it was reproduced, it would be recognized, whereas it could not be recognized, except in a very vague way, in the second case. It is a well-known fact that any particular sensation or element of a complex can be recognized or singled out more easily in the complex if it has been presented previously by itself, as, *e. g.*, in the case of a compound clang, any note in it can easily be singled out by the attention if previously given separately. And this is as true of the other senses as of the auditory.

Although the sensation attending the ear movement was

identified as soon as the least response was effected, yet at the same time the subject was perfectly aware that only a small part of the total sensation came from the ear; and in repeating the innervation he would try to have a greater part of this complex sensation experienced from the ear. In thus trying harder to move the ear farther the brow would also be raised higher, the jaws set more tightly, etc., so that relatively the sensation experienced from the ear may not have been greater than before, but absolutely it was very much greater. The process continued until the ear muscle could be contracted to its maximum. The ear was definitely located with reference to the other muscles which were innervated with it. An association was made between the sensation from the ear and the sensations coming from the whole group.

As soon as the sensation arising from the movement of the ear was associated with the concomitant sensations of muscles close to it, over which there was already definite voluntary control, there was a basis for learning the voluntary control of the ear. To put this into a general statement would be to say that in order to acquire voluntary control of a new muscle it must be associated with the movement of some muscle we already know. This principle, if it can be shown to be true, which, it seems to me, our facts would make indisputable, is entirely consistent with a well-known pedagogical principle of association on the intellectual side, namely, that we can only get hold of a new thing in terms of what is already known. If we cannot associate it in any way with our past experience it can never have any significance for us.

We now come to the second part of the process (stage III. previously described). We have gotten a hold on the ear by association with muscles known. The next thing to do, now, since this association is made and the ear can be made to respond to an effort of the will, is to break up this association and move the ear independently. In the accomplishment of this the attention is the all-important thing. This sensation which satisfies the effort of the will, *i. e.*, the one attending the contraction of the retrahens, becomes the thing upon which the attention is focused. The associated muscles are gradually

relaxed. They respond less with each trial until finally only the ear responds. The rapidity with which this is accomplished is entirely proportional to the concentration of the attention. The attention directed to one activity tends to inhibit the other activities which may be in progress at the time. This is a conclusion which we could not help but draw from our facts and introspection. It was the experience of all that the more completely the attention could be concentrated on the movement to be made the greater was the attending success. When all the attention was put upon the movement while the brow was forgotten the movement could be made without any of the concomitant brow movement, whereas when part of the attention was directed on inhibiting the brow movement while the other part was directed to the ear movement both ear and brow responded together.

Our conclusion as to the nature and importance of attention is corroborated by many observers. Mrs. Moore[1] has noticed the same fact with reference to children, and we will quote what she says: "Inhibition was first induced by a sense stimulus, which in drawing attention into another channel caused a movement already in progress to cease." Preyer[2] says that inhibitions are positive willings in another direction, and he lays it down as a pedagogical principle not to have any 'Don'ts.' Inhibition, Ribot[3] furthermore holds, is connected with the attention process, and when we attend to one idea or impression there is a momentary inhibition of the other ideas or impressions. Exner[4] also gave it as his opinion that inhibition (Hemmung) is not by a positive effort of the will shutting out a movement in process, but it is rather taking the attention away from the movement and directing it into another channel. James is certainly not far wrong when he says that one process inhibits another by appropriating its molecular energy. The familiar facts of everyday experience are sufficient to show the pertinency of the above

[1] Kathleen C. Moore, 'Mental Development of Child,' PSYCHOL. REVIEW, Monograph Sup., Part I., Section 3.

[2] Preyer, 'Infant Mind,' p. 64.

[3] Ribot, 'Psychology of Attention.'

[4] Exner, 'Psychologische Erklärung der psychischen Erscheinungen,' p. 72.

conclusion, *e. g.*, trying to banish unpleasant thoughts only makes them more vivid. We banish evil thoughts by substituting others for them. It implies a moving away from them of the attention. If we try not to do a thing and keep our attention on the 'not doing,' we are most likely, in spite of ourselves, to do it. A bicyclist in keeping his attention on a stone he wishes to avoid is most likely to strike it.

Another, an involuntary consequence of attention and attentive concentration, is the arrest of the bodily movement or limbs. When we fall into a deep study while out walking, *e. g.*, we ease our pace and may even come to a standstill. "If an idea suddenly seizes upon us with full force, we interrupt what movement we may be making, quite automatically."[1] Concentration of the attention has, accordingly, from all these facts here enumerated and from the facts of our experiment, an inhibitory effect on the motor innervation of muscles whose activity is not implied in the attentive act.

The importance of attention in learning voluntary movements, as well as in learning anything else, is everywhere emphasized. Recent experiments made by Bryan and Noble[2] on the learning of the telegraphic language serve as a good illustration. "It is intense effort that educates. Each step in advance costs as much, and indeed more, than the former. Men do not as a rule become experts, because they will not make the painful effort necessary." Having now stated the facts, and the processes and factors involved in the learning of voluntary movements, from the psychological side, we will proceed to explain the same facts from the physiological side, for it is only when explained from their physiological basis that these facts can have any real significance.

The retrahens, notwithstanding the fact that most persons have no voluntary control over it, is adequately supplied with motor and sensory nerves (see Fig. 2). It is probably because ear movement has become of so little importance to man, that the motor tracts supplying the ear have become sealed, so that none of the molecular energy (to borrow Spencer's terminology),

[1] Külpe, 'Psychology,' pp. 433 ff.

[2] 'Telegraphic Language,' PSYCHOL. REV., Vol. IV., pp. 50 f.

which is diffused throughout the organism of the infant in proportion to the physiological openness of the different motor tracts (this openness being phylogenetically determined), is discharged through them. Many who believe in the transmission of characters hold that it is largely a matter of nervous susceptibility—that the resistance a nerve offers to this original discharge throughout the nervous mechanism is in inverse ratio to the amount of use it has been to the race. This hypothesis, if it be true, would explain why the random movements of the infant, *e. g.*, those general movements of the hands towards the mouth, etc., so closely resemble those necessary to survival in the struggle for existence. The child, we might say, is born with general motor tendencies, which from the direction of these motor discharges reflect in a large degree the history of the race.

The ear muscles, on account of their long uselessness, are among the few muscles in the human body that have not, at some time or other, been innervated. No motor impulse has ever passed over them on account of the relatively great resistance they offer, and therefore the first thing to be done is to force an opening (*Bahn machen*). The first movement of the ear was effected by an intense innervation of the muscles (*i. e.*, those already under the control of the will) physiologically connected with it by means of the same motor nerve. Referring again to our illustration of the anatomy of these parts (Fig. 2., p. 480), it will be seen that the same motor nerve (facial) which supplies the head and face supplies also the ear. By vigorously raising the brow the ear also made a slight response to the impulse. This may be explained as follows: We have a vague idea of the location of the ear through sensation in general; we concentrate our energies in that direction, which results in a motor discharge in the same general direction, but the discharge follows the paths of least resistance, and all the muscles supplied by this nerve and its branches will be contracted in proportion to the ease with which the impulse is diffused through the different branches. The ear may not be reached at first. Up to a certain point the additional discharge of motor energy finds a way of escape more easily through the old channels than through the new, but beyond this point a part of the impulse is forced

through the new tract, and each time the process is repeated more of the impulse is discharged through this line until it is so well opened that the muscle can be contracted to its maximum. As the energies are now more definitely centered on the reproduction of the specific sensation of the movement originally sought for, more and more of the motor impulse is directed through the channel which produces this desired sensation, and continually less leaks out through the old channels, until eventually the whole impulse is so directed as to produce specifically the movement desired, and this is *voluntary* control. The concentration of the energy upon one channel of discharge, or one course of action, seems to have one of two effects or both. It either lessens the resistance along the line of concentration, or it increases the resistance of the other possible paths, and it is not improbable that it does both at the same time. Wissler,[1] in a series of experiments on the diffusion of the motor impulse, found that, *e. g.*, when a motor discharge is directed to the extreme accessory muscle of the arm, the law of diffusion is, primarily, to the muscle directly innervated, and secondarily to the adjacent related muscles in the order of their distance anatomically from the muscle innervated. These facts seem in direct support of the view above advanced, namely, the importance of the attention; the resistance is much less along the line in which the discharge is directed, *i. e.*, toward the fixation point of the attention, and the farther away from this point any process is, the greater is the inhibition of the motor tracts supplying it.

We have no reason to believe that the physiological processes involved in the learning of the ear movement are not exactly the same as those in the acquirement of any other movement, as, *e. g.*, that of the hand. As we have already so strongly emphasized, the learning of the movement is largely a matter of the attention, and, in general, it is just in proportion as the learning of a movement is a vital thing that the bodily energies are concentrated upon it. A child, *i. e.*, brings together all its force and directs all its attention to learning a movement which is essential to its freedom. All who have ob-

[1] PSYCHOLOGICAL REVIEW, Vol. VII., Jan., 1900.

served children (and this is a fact emphasized by all genetic psychologists) have noticed how persistent they are in learning a movement. In learning to walk a child is usually so much absorbed in the attainment of the end that the incidental bumps and falls, however severe or painful, are hardly noticed. When once a successful movement is made, the attention is so engrossed on a repetition that everything else is for the time forgotten. In acquiring voluntary control over the ear this fact has particularly impressed itself upon our mind, and in all probability voluntary control over each and every movement of the body must be acquired in the same manner. And while, in the one case, since the acquiring of this control is a matter of salvation to the organism, the attention would naturally be drawn upon it; in the other it would be merely a matter of accomplishment. The attention required to learn would be arbitrary and for that reason could not be so close and consequently the movement could not so readily be acquired, provided, *i. e.*, that the attention is the important factor in learning, which it seems, in view of the facts, none can doubt.

We will now look at our facts from a more general standpoint and show their bearing on the genetic theories above stated (Sect. IV.). We cannot do better than accept the general hypothesis that the child begins its existence with a profusion of random movements which have no definite adjustment and no fixed relation to the stimulus, but which serve to bring into play the voluntary motor mechanism and supply consciousness with experience and thus constitute the psychical initiative to voluntary movements. It is generally argued that all movements, before they can be voluntarily produced, must first be given in a random or reflex way. Our experiment shows that at least one movement, namely, that of the ear, can be acquired without first having been given in this way. But some movements, however, as we have seen, must be given in this random and reflex way in order to form a basis for voluntary movements, because the unknown muscle (in our case, the retrahens), if we are to get control over it, must be associated with muscles already known, *i. e.*, with those into which the motor impulse can be discharged voluntarily. This association is made by

overcharging the old channels through the vigorous effort to reach the new muscle, and thus forcing part of the impulse into new channels, among which is that supplying the muscle for which the impulse is intended.

It is universally accepted that the child, in addition to random movements, has certain reflex movements which are due to a certain definite connection between the motor and sensory paths, *i. e.*, the physiological circuit[1] is already completed so that the organism will respond to a specific stimulus in a certain definite way.

It is generally admitted that the random movements are due to a diffused molecular discharge throughout the motor nerves, in which the greater part of the impulse goes over the tract which has been of most use to the race. It must be assumed that the movement which has best served the race will likewise be useful to the individual. And, as Jastrow[2] says, "The existence of habits implies an environment sufficiently constant to repeatedly present to the organism the same or closely similar conditions." Habit implies a definite connection between certain motor and sensory tracts. It also further implies that certain tracts are so well defined that the motor discharges along them take place almost automatically. Under an unchanging environment, *i. e.*, the movements which have served the race best and consequently which receive the largest part of the motor discharge in infancy are soon found to be the means by which the child must realize itself, and are, therefore, repeated on account of the accompanying gratification. If, on the other hand, there is a constantly, but gradually, changing environment, the diffused discharge will procure the adaptive movement. Any part of this diffused discharge (in a modified environment), or of the movement produced by the discharge, which effects the gratification will immediately attract to it the attention and consequently as the process is repeated will gradually appropriate more and more of the general discharge, until only the part which secures the gratification is moved, and that channel becomes relatively the course of least resistance.

[1] Dewey, 'Reflex Arc Concept,' PSYCHOL. REV., Vol. III.

[2] *Popular Science Monthly*, Nov., 1892.

But no movement which brings gratification to the organism or which is of any considerable use to it can be due to the single contraction of any individual muscle or group. An act in an organism so complicated as that of the human being implies the working together of a number of muscles, each performing its part at the different stages of the act. How this is done is not difficult to understand from our point of view of the attention. In all our acts the movement follows the attention. The attention does the switching. This is especially true while learning a movement. A good illustration is learning to swing clubs. When learning to alternate, if the attention is directed to the one hand the other either stops or goes in the direction to keep the body balanced, or there may be a symmetrical discharge to the two hands. In the last case the attention is divided between the two hands, and the impulse goes out in two directions. In learning and coördinating new movements, we do not shift our attention rapidly enough, and in consequence the other movements which are necessary to complete the adaptive act are inhibited. Each part of a complex act is repeated until the sensori-motor circuit is well defined, and the movement as a whole can only be accomplished when each element, once put into operation, tends to act out its part automatically or with very little attention to each part to be operated. Ward[1] says: "The common factor, in all voluntary action alike, seems to be a change in the distribution of the attention under the influence of feeling." And while control is being acquired the power to relax a muscle is the power to shift the attention away from it either to some other part of the activity or to the antagonistic muscle.

In closing this discussion we cannot do better than to interpret our facts in the light of the theories above stated. Assuming, therefore, that the original motor discharge hypothesis and the hedonic concept are correct, and also assuming that the race is, and has been for many generations, exposed to a constant, or at least only gradually changing, environment, the facts of our experiment could only be interpreted in the following way: The motor nerve elements evolve from a central system, from funda-

[1] *Mind*, Vol. XII., p. 64.

mental to accessory. The 'primordial impulse' is discharged along the paths of least resistance, which paths are phylogenetically determined. This discharge along the race-defined tracts causes movements which are beneficial to the organism and hence are attended by pleasurable sensations. These sensations and the movements which cause them are associated, *i. e.*, physiologically speaking, the sensori-motor circuits are completed, so that the process can be reversed, *i. e.*, when the sensation or the idea of the sensation of any one of these movements is given to consciousness there will be the coördinated motor response. But only those movements can be made by the idea of the movement which have previously been made by a motor impulse, *i. e.*, a stimulus will not cause a motor response except where the circuit has already been completed and the central association made, and this association could only be secured (except in the case of certain reflexes where the circuit is already completed at birth) when the movement was first caused by a motor impulse. It is quite certain that motor nerve elements evolve from the central system, from fundamental to accessory.

In the case of a gradually changing environment, which must be the case with man, the adjustment is not so simple as in the above case of fixed environment. In this case the discharge along the path of least resistance would not effect the happy movement and the gratifying sensati[illegible]but [illegible]ome part of the general movement caused by the diffused discharge would contain the adaptive element. This would be cognized as pleasurable, and the energies concentrated upon repeating it and prolonging the gratification, until a definite association were made and the circuit were completed and become well defined. This is the way movements are specialized, adapted, and coördinated —most of the bodily movements were originally given through this diffused discharge, and the general sensori-motor paths are thus completed. Nearly the whole process from the impulsive child to the manually skilled adult is one of adaptation through analysis, as above described.

But we have seen that not all the movements possible to come under voluntary control were primarily given in this general

movement, or movements, caused by the diffused motor discharge. How control is acquired over such a muscle we have already described in detail, and we need here only summarize it in a general statement. In this case only a vague, indefinite idea of the location of the movement to be acquired is given through general sensation (visual, auditory, cutaneous). An impulse is sent out in the general direction, which results in a general innervation. In the response there may be an element of the movement sought for. As soon as the movement is effected, however feebly, the sensori-motor circuit is beginning to be made. The sensation will also at the same time be cognized and the effort made to increase the sensation. When an impulse has once actually reached the muscle the process from that point onward is one of segregation, specialization, and analysis, as described in the case of the adaptive movement.

PSYCHOLOGICAL LITERATURE.

Ethics. Volume III., *The Principles of Morality and The Departments of the Moral Life.* By WILHELM WUNDT. Translated by MARGARET FLOY WASHBURN, Ph.D. New York, The Macmillan Co. Pp. 304. $2.00.

While the two earlier volumes of Professor Wundt's 'Ethics' do not fall within the province of this review, their more important bearings on Volume III. will be considered, in order to make explicit the logical framework of the author's system. Two methods are recognized by Professor Wundt, the speculative and the empirical. The former looks upon fundamental moral laws as original and perfect possessions of the mind, to be discovered by insight and brought to clear consciousness. The latter undertakes, but seldom carries out, a full investigation of the field of morality, as a basis for the discovery of moral laws. Neither method is satisfactory when used alone. Every speculative system overlooks a multitude of true and important facts. And empirical systems cannot discover fundamental ethical principles, but only pave the way for their discovery; "the actual discovery is the task of speculation" (Vol. I., p. 17). Both methods must accordingly be combined, the empirical being used first, to 'furnish the materials with which speculation erects its structure.' The empirical method reaches to "direct abstractions and inductions from experience. Speculation begins, on the other hand, whenever hypothetical elements enter into the formation of our ideas—elements not derived from experience, but introduced into it under the influence of the logical requirement of unity of thought" (Vol. I., p. 18).

In making thorough and conjoint use of the two methods in the order indicated, Professor Wundt has made a noteworthy advance. By his thorough survey of the facts of morality he has done much towards establishing Ethics on a scientific basis, and making it possible to apply critical tests to his groundwork and conclusions. For, as long as Ethics is based on the analysis of facts selected by mere insight, instead of by some explicit process of reason that can be subjected to objective tests, no conclusion of proper scientific authenticity can be attained. Such results of insight are little more than judgments of experts, for on such judgments they are founded, and their weight

depends upon the wisdom of their authors. As Kant and, indeed, Socrates taught, scientific conclusions are the result of explicit reasoning processes that are open to critical testing. Nor is there any doubt that the speculative process, as defined by Professor Wundt, is a necessary supplement. The gathering and testing of facts, abstraction, and generalization, are aimless and inept processes, unless unified and systematized by hypotheses.

In carrying out the method decided on, Professor Wundt devotes Part I. (Vol. I. of the English translation) to 'the moral consciousness of man, as it finds objective expression in the universal perceptions of right and wrong, and, further, in religious ideas and in custom' (Vol. I., p. 19). Here he discusses the light thrown by language on ethical ideas, and the relations of religion, custom, and law to morality. The volume is interesting and valuable, and has already been reviewed in these pages (Sept., 1898). In Part II. (Vol. II. of the translation) occidental ethical systems are considered historically; first those of Greece and Rome, then the Christian systems of the patristic and scholastic periods, and finally modern, chiefly continental, systems of ethics. The volume closes with a thirty-page classification and criticism of ethical systems that is thorough, considering the space it occupies, and valuable. These few pages suggest the errors that Professor Wundt plans to avoid in his own system. Parts III. and IV. deal respectively with 'the *principles* on which all judgments of moral value rest,' and with 'the *applications* of these principles to the great provinces of the moral life' (Vol. I., pp. 19–20). In Parts I. and II. the empirical method is used to reach facts and generalizations therefrom; in Parts III. and IV. the speculative method is used to discover fundamental moral principles and rules of application.

And here it must be confessed that, promising as Professor Wundt's plan of treatment is, the broader features of his execution are disappointing. Considered together, the four parts can only be described as disjointed. Parts I. and II. are not closely articulated to Part III., nor is Part III. to Part IV., as will appear when the latter is taken up. After a study of the facts of the moral life covering some three hundred pages, the author's conclusions are, that "morality expresses itself in *antitheses*" (Vol. I., p. 326), *e. g.*, approval and disapproval; that "certain goods are regarded as morally desirable whose enjoyment promises an *enduring* satisfaction" (Vol. I., p. 327); that 'there are two fundamental *psychological* motives' (Vol. I., p. 328), reverence on a religious, and affection on a social basis; that the principle of the heterogony of ends is a matter of universal experience; and a few

others of lesser significance. Other results there are of a less general character, but I have been able to find none that throw light on the nature of morality as a whole, or that give, or are thought by Professor Wundt to give, decisive evidence for or against any principle reputed moral. And the insulation of Part II. is almost as complete. The few concluding pages of criticism of ethical systems do not rest explicitly upon any portions of Part I., nor upon any principles discovered earlier in Part II. They are Professor Wundt's independently reasoned opinions of the failings of ethical systems.

This does not mean that Parts I. and II. have been of no assistance to Professor Wundt in writing Part III. These investigations must greatly have broadened and deepened his ethical views. Consciously and subconsciously they must have changed his opinions in matters of vital import. Regretable is Professor Wundt's failure to observe the nature of the processes that led from the facts earlier studied to the later conclusions, or, if he observed them, his failure to set them forth, so that their validity might be tested. It is well to saturate the mind with pertinent facts, and then speak, or otherwise act, without further ado; in practical affairs that is often the best plan. But science insists on scrutinizing every link of the chain that stretches from facts to conclusions. No chain is stronger than its weakest link. But the strength of Professor Wundt's chain of inferences cannot be estimated, as many important links are submerged and out of view.

Part III., which occupies nearly two-thirds of the volume before us, is divided into four chapters, entitled respectively, The Moral Will, Moral Ends, Moral Motives, and The Moral Norms, the author's designation for the fundamental rules of right action. The first chapter furnishes the psychological foundation for the later ethical structure, and centers about a very interesting discussion and characterization of the 'social will.' This puzzling and contentious conception plays an important rôle in the author's system, which cannot be understood without it. And, first, Professor Wundt does not hold that there is one consciousness enveloping the component individual wills that, in their unity, make up the social will. The social will is a totality of individual wills made one by unity of purpose. Yet, in a sense, the social will has self-consciousness. In some cases, at least, there is a consciousness, common to the component individual wills, of purposes shared by them in common; whether or not individual wills ever actually coöperate for common purposes without consciousness of unity of purpose, and whether or not such a totality of wills would constitute a social will, Professor Wundt does not say.

In general it may be said that the author is most anxious to emphasize the solidarity, in fact, the conscious solidarity of the social will; many times, for instance, he insists that the individual will knows 'itself to be an element in a total will which supports it in its motives and ends,' as he expresses it in a striking passage (Vol. III., p. 27). I have noticed no mention of the organization of the social will; certainly there is no discussion on this point.

Professor Wundt also shows that social wills differ in scope, *e. g.*, the family will, the wills of associations, the wills of the state and of humanity; and holds that the state is a person, because of 'multiplicity of ends and autonomy of will' (Vol. III., p. 284).

The last two sections of the first chapter discuss freedom and conscience. The former discussion is based upon Professor Wundt's distinction between mechanical and psychical causality, the latter alone being shown to reign over the will; this epistemological discussion taps some of our deepest problems. The gist of the author's opinion on the main question is packed into his pregnant opening sentence, "Freedom is the capacity of any being to be determined in its action by a reflective choice between different motives" (Vol. III., p. 37).

In the author's words, gathered from several contexts, moral conscience may be said to be made up of ideas, immediately associated with the emotions of approbation and disapprobation, that as a whole constitute imperative motives, and pass judgments upon the agent's motives and character. Four imperative motives are distinguished, the two motives of external and internal constraint, and two free imperative motives, permanent satisfaction and the moral ideal.

Professor Wundt considers three main orders of moral ends, the individual, the social, and the humanitarian, which form a hierarchy of ascending worth. But individual ends are never 'true objects of morality.' The preservation, the satisfaction, even the perfection, of self is a true moral end only when sought in the interest of social or universal advantage. And here begins a dialectical process that rules the entire discussion. If the agent's well-being is not a proper moral end, the well-being of other individuals bears the same taint. Thus "there remain two social ends as the true objects of the moral will,—public welfare and universal progress" (Vol. III., p. 80). Nor will the dialectic permit these two ends to be interpreted in individualistic terms. Public welfare is not 'the sum of the welfare of all individuals,' it is not 'a maximum of happiness,' any more than universal progress is 'the progress of as many individuals as possible' (Vol. III., p. 81). "The individual, however perfect and happy, is but a drop

in the sea of life. What can his happiness and pain signify to the world?" The end is not individual but social, and, pushing it to the limit, not this or that society, but the supreme society embracing humanity. The well-being and progress of the universal social will is the genuine and supreme moral end. So I interpret Professor Wundt; his meaning here is very difficult to interpret accurately. Individual well-being and progress cannot be summed, so long as the individuals are isolated atoms, but organize them into social wills and consciousnesses (as earlier described), and there supervene unitary objects of endeavor. "The universal psychic products [*e. g.*, the state, science, art, universal culture] of human society" (Vol. III., p. 86) are the objects of our supreme moral concern.

Happiness enters negatively rather than positively into the end, as conceived by Professor Wundt. If his pursuit of moral ends did not bring in happiness, man could not maintain it. But happiness is not a proper object of moral endeavor. The dialectic shows it; collective happiness is a nonentity, since happiness cannot be summed; besides, teleological activity is man's essential trait, and the only proper object of his concern. The second reason is strong, but the other two will not, I venture to believe, bear scrutiny. As to the last, teleological activity is no doubt the *essential* aspect of man's nature, but there are other aspects, *e. g.*, feeling, and they are surely not matters of indifference, even to the social will.

Profound is Professor Wundt's insight that the *ultimate* end is an ideal of the reason that cannot be defined. The progress in perfection of objective psychic products, *i. e.*, forms of social will, is only our proximate end; to what perfection progressive mankind should ultimately attain, it will forever be impossible to determine. Every improvement of the social will leads, because of the nature of will, to the opening of new possibilities.

In order for an act or a disposition to be moral, the motive must be moral as well as the end, and with moral motives the third chapter deals. The earlier half distinguishes motives of perception, motives of the understanding, and motives of reason, and points out that the ultimate spring of immorality is egoism. The second half deals with immoral motives. Especially good is the discussion of the conditions of immoral volition. Immorality cannot be explained, Professor Wundt shows, either by extreme individualists, who assert reality of the individual but deny it of the social will, or by extreme universalists, who deny reality of the individual and assert it of the social will. If either of these alternatives were true, immorality would become a

matter of quantity, or else a mere appearance, because of the nonentity of the individual. But both are equally real, though the individual is of lesser moral worth, and, "as man's whole psychical existence is bound up in the society to which he belongs," it is plain that, "the will is moral, * * * as regards its character or disposition, so long as the motives that determine it coincide with the ends of the social will" (Vol. III., p. 110). To be sure, social wills, as they are brought forth in historical development, are fallible, but actual wills are only relative, and the absolute social will is "an idea of the reason, by which we must always suffer ourselves to be guided" (Vol. III., p. 111).

The final chapter of Part III., which covers fully seventy-five pages, opens with the statement that a norm "designates which one of various possible kinds of action ought to be performed" (Vol. III., p. 138). But the discussion is concerned only with fundamental norms, which cannot be derived from any more general principles. Like ends, they are classified as individual, social, and humanitarian, each of which has a subjective aspect, prescribing items of character, and an objective aspect, prescribing kinds of action. Professor Wundt's formulations follow: individual norms, "Think and act in such a way as never to lose respect for thyself," and "Fulfil the duties to thyself and others, which thou hast undertaken"; social norms, "Respect thy neighbor as thyself," and "Serve the community to which thou belongest"; humanitarian norms, "Feel thyself to be an instrument in the service of the moral end," and "Sacrifice thyself for that end which thou hast recognized to be thine ideal task." Inspiring and nobly conceived are these norms, but no reasons are given for regarding them as exhaustive or accurate.

In view of the multiplicity of norms, their occasional conflict is inevitable, and the author finds it necessary to lay down a rule of preference. The hierarchy of moral ends provides the rule required: "When norms of different orders contradict each other, that one is to be preferred which serves the larger end" (Vol. III., p. 140). Professor Wundt sees that this rule may be misconceived, but undertakes to show that, properly understood, it is unconditionally decisive. In this endeavor his success is doubtful. Quite literally, at every moment there are conflicts of ends and of motives, and only by the organization of life on some less mechanical principle can such problems be solved: our familiarity with social organization blinds us to the complexity of the problem.

The chapter, and Part III., close with a thirty-page discussion of legal norms, which well deserves consideration, did space but permit.

Part IV. contains Professor Wundt's views on the practical problems that arise in the different departments of the moral life, under the captions, The Individual Personality, Society, The State, and Humanity. Very little, if any, explicit reference to the principles of morality earlier discovered is made. But the moral significance of such possessions and institutions as property, intellectual cultivation, associations, the state considered as a financial, a legal entity, etc., and international law, are interestingly discussed on the broad basis of the author's ethical outlook. Some of his more striking views may be briefly suggested. It is the duty of the state to see that private labor has the freedom requisite to a share in intellectual interests. In natural science, political science, and history all should be educated; in philosophy, philology, and the history of civilization only the higher grades of mankind. Woman is intellectually capable of certain higher kinds of employment, but she is out of place in politics, and science has been shown not to be her true province.

Criticism, which may well be at fault, is the most fitting tribute to a scientist of Professor Wundt's standing. Appreciation is a matter of course. Two contributions, if no others, would, I venture to believe, place his work among the most important of its kind and century; first, the modern formulation of the conception of the social will, the insistence, as against narrow individualists, on its reality, and the happy employment of this profound truth to solve several knotty problems; secondly, the insight and demonstration that the moral task of mankind is ultimately indescribable,an inspiring ideal of the reason. The English translation of such a work is to be warmly welcomed, because of its difference from our other authorities and because of its intrinsic excellence.

The translator of Volume III. has done her work surprisingly well. An unusually idiomatic and involved German text has been rendered into equally idiomatic and decidedly readable English. I have noticed only three blemishes, an unfortunate use of 'when' and 'where' on pages 91 and 118 respectively, and the mistranslation 'self-regarding' towards the bottom of page 100. The volume has an index, which, however, might well be fuller.

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Beiträge zur Philosophie des Gefühls. Von F. RITTER VON FELDEGG. Leipzig, Verlag von Johann Ambrosius Barth. 1900. Pp. vi+122.

Conceptions of the world as force and will, as idea, thought and reason, have been entertained in every age; but the author, in this

confessedly dogmatic and polemical treatise, develops a view of reality in which feeling is fundamental, and he understands feeling in a somewhat unusual sense of the word. "The aim of modern philosophy is to achieve a synthesis of the world as matter and the world as consciousness; but until recently philosophy has only tried to realize this synthesis *in abstracto;* and yet, exactly this synthesis is more concretely given, as an actual feeling within my consciousness, than any other in the world. Empirically to trace out, in this feeling, the dual basis of real and ideal, of objective and subjective being, is the chief problem of every psychology which seeks to go beyond the achievements of the past; and, as such a psychology necessarily stands in close relation to philosophy, it must also be one of her greatest needs" (27). Philosophy is here repeatedly arraigned for trying to realize this synthesis by a definition and elaboration of abstract conceptions, instead of seeking for it among the given facts of experience. The abstract dualism of thinking and extended substances apprehended by the 'understanding,' with which Spinoza struggled, was certain to fail. Spinoza's 'understanding' is a mere *x* which cannot be further deduced, because it appears at the end, instead of the beginning, of his system, and cannot be derived from the notion of 'thinking substance.' Essentially the same difficulty shadows every philosophy which does not find in consciousness a concrete synthesis of subject and object, of thought and thing. The demonstration of an in itself monistic and self-identical subject-object alone can overcome this difficulty (28). Fichte's logical 'ego' and Hegel's 'Spirit' are not centers of consciousness in the concrete, but its outermost abstract periphery; Schopenhauer's 'will' stands to consciousness in a relation of exclusion; Schelling's 'unconscious spirit,' which is in the main synonymous with von Hartmann's 'unconscious,' is absolutely useless as the foundation principle of a philosophy of consciousness. The mistake of all these philosophers (except perhaps Schopenhauer) lies first in their beginning with an abstract conception instead of a concrete self-conscious experience, and secondly in their placing self-consciousness, as an ideal product, at the end of a biological development, instead of at its beginning. These fundamental errors rest at bottom upon a confusion of self-consciousness and self-knowledge. The latter comes late in the process of growth; the former is the germ of all growth. The former is a feeling, the latter an ideal product. "Self-consciousness is accordingly the original state, the root of all mental life; self-knowledge, its flower" (p. 30 f.).

There exists an important difference between a philosophy which starts with abstract conceptions and proceeds analytically, and a philosophy based upon actual experience: even professedly experimental philosophies sometimes start with an abstract definition of experience which vitiates their contents; but the time for such philosophies is, in the writer's judgment, once for always passed. "From now on one will have to seek for the basis of the world-order no longer in airy abstractions, but rather wherever the world-fact (Weltthatsache), in its most concrete form, is to be met with; and I do not conceal the opinion that this place is our feeling" (47).

In the preface, the author refers to the change which has taken place within recent years in the methods and aims of German philosophy, and expresses the hope that in the near future the Germans will again look up to the clouds for that which exists in the clouds. "The German to-day sees the smallest pebble along the way, but he no longer looks off to the horizon toward which he nevertheless journeys." He must seek a monistic view of the world; and this differs from pantheism in that the latter is always anthropomorphic and simply pushes the great problem of human nature one step further away, while monism seeks in nature as well as in man for the one explanatory principle of things. The mistake of all existing monistic philosophies lies in their starting with a dualism of abstract conceptions, matter and consciousness, whose opposition they can only resolve by some illogical *tour de force* which leaves us unsatisfied. The philosophy of feeling regards matter not merely as an abstract conception but as given in our feeling; and here it exists, not separate from our subjective being, but rather in identity with it. The essence of feeling consists in the identity of a subjective with an objective, and feeling is 'the original actuality of ourselves.' The philosophy of feeling teaches these two things, that in metaphysics feeling alone, out of the totality of consciousness, must be considered; and that in feeling these two fundamental data, matter and consciousness, are not simply given, but united in perfect identity.

The *Kritik der physiologischen Psychologie* deals chiefly with the views of Ziehen, but regards these as illustrating the general position of this new branch of science. He objects to its notion that consciousness begins with sensation—to the neglect of reflex and automatic processes. "In fact," he writes, "consciousness is to this psychology ('without a psyche,' to quote Lange) merely a sort of physiological cadence, the tip of a handkerchief, which protrudes from behind out of the physiological professor's coat-pocket, and of

which he would be ashamed if he did not find it so absolutely necessary." The tendency of the physiologists to despiritualize the ego is evident here. They first rob the ego, bit by bit, of its being, until nothing is left but an internally barren cell-heap: then proceed with infinite pains, bit by bit, to refill the empty cells. Secondly, he objects to Ziehen's account of the origin of sensation out of its stimulus. This view is contradicted by Ziehen's own doctrine of a pyschophysical parallelism, according to which a sensation is a double thing —a physiological process with a psychic correlate. Likewise, physiological psychology reduces the attention to an association process dependent upon outer stimuli, and we become thereby our own spectators without will and without initiative. Virtues are simply ideas with positive feeling-tone; vices, ideas with negative; if there be an absolute good or evil, it must be the function of metaphysics and religion to show it. For Ziehen the problem of the freedom of the will sinks to a mere farce. Finally, the ego which physiology seems determined to drive out of psychology entirely, is still retained in altered form. My body moves itself in response to stimuli, and I become aware of those movements for myself; but what am I? A center of self-consciousness which cannot be identified with the body. "Thus physiology, against her will, has reached a spiritualistic hypostasis which, while it suspends the fundamental relation of identity between body and soul, contradicts all that, up to this point, she has taught." The physiological conception of the psyche is untenable.

In the essay on the Origin of the Psyche, the positions are taken that the individual psyche has no beginning in time; that it is originally a feeling, and that this feeling is ultimate reality. In the views of Du Prel as to the nature of the psyche, the author finds much that agrees with his own. Münsterberg and Otto Schneider are quoted in support of his views as to the nature of the ego and its relation to the content of consciousness. The Self of which we are conscious and the content of consciousness cannot be separated. The former without the latter is a ghostly abstraction; the latter without the former, a contradiction.

As to the limits of the Objective and the Subjective in the Knowledge Process, the author holds that every phenomenon is entirely objective or entirely subjective, according to the point of view we take, the two points of view being grounded in the unity of subject and object—which self-consciousness always involves. The two phenomena, the object and subject, do not completely correspond in the

individual finite consciousness, as the fact of a threshold of sensation, for example, shows; but there is no reason for saying that the stimulus, even in this case, is not psychic. On the contrary, were it not so, no transition from a non-psychic stimulus to a psychic process would be conceivable; even the spatial separation of the two exists only for consciousness. The boundary separating subject and object should not be conceived as a surface standing at right angles to the line of perception, but as within the process of knowledge running parallel with it. "Upon a purely empirical basis we have to regard the intelligible and physical, the objective and subjective causal series, as two lines running parallel with each other and by no manner of means constituting, as physiology asserts, their own prolongation" (74).

The next essay deals with The Ethical Movements of the Present, which, while preaching morality heroically, fails to ground it philosophically. The ethical movement says, "Seek to realize your highest duty, because it is the crown and glory of humanity"; but when asked why humanity should be made an end, this movement replies: "Because humanity is morally good." It grounds morality in humanity and humanity in morality! Moreover, the law of motivation is here underestimated, or rather completely misconceived. Morality is conceived to be a matter of practice and education; man must be trained to goodness. But in this they forget that goodness can be realized only where profound conviction of the necessity of goodness exists, and it is never the result of mere practice. "No stone falls without a cause and no man acts without a motive" (87). The presupposition of all morality is belief in a supersensuous order, belief that morality is no mere social agreement or law of habit, and that, on the contrary, it has a superhuman as well as human validity (90).

So far we follow the author's discussion with great interest; but in the next essay, on Palingenesis from a Psychological Standpoint, he enters a field of speculation in which, as it seems to the reviewer, the weakness of this 'philosophy of feeling' becomes apparent. Each psyche is a synthesis of an empirical and a transcendental element. "Every part of the soul known to us is a self-identical being which is nevertheless joined to a second, unknown part whose identity we, from a psychological standpoint, can neither affirm nor deny"; and this simply opens a free field for the doctrine of the reincarnation of the soul upon earth. The transcendental, unknown half of the soul is the bearer of a transcendental individuality which runs through a number

of empirical reincarnations, because it combines each time with a new empirical half-soul. No knowledge is vouchsafed us, however, as to the nature of the bond which realizes their union, and no bridge of memory enables us to pass from one stage of existence back into previous stages.

The last essay of the book deals with the relation of this doctrine to the fundamental principles of morality. To be true to one's self, in the highest sense, means altruism, because this highest self is transcendental and universal; and yet, such altruism is but the highest form of egoism; and thus, a synthetic basis for morality is reached, enabling us to combine the ethics of hedonism and the ethics of reason with the ethics of perfection or eudemonism.

This is not the place for a careful discussion of this theory of reality; but we may be allowed to say something as to the impression it has made. That philosophy should start with the concrete fact of self-consciousness, rather than with some abstract logical construct, many are ready to assert and have asserted; but why conceive self-consciousness as pure feeling? Were there no element of discrimination in it, the rise of knowledge would be inconceivable. All the author's criticisms of the type of monism represented by the physiologists can be turned against this view by a simple substitution of terms. Moreover, why regard self-consciousness as pure feeling rather than as self-knowledge? The author will answer, because it is a feeling *before* it is a knowledge; but in doing so, he first assumes that primitive self-consciousness is pure feeling, and then attempts to base philosophy upon the lowest rather than upon the highest stage of evolutionary process. This demands that a missing and very careful account be given of the relations of the latter to the former. The deduction of knowledge and volition from pure feeling is as impossible as their deduction from the logical concept of matter. In other words, feeling as the author uses the term seems to us as much an abstraction as matter or thought. My self-feeling exists at the primitive stages of evolution no more than my self-knowledge or self-realizing volition. Many of the author's criticisms of current philosophical, psychological and ethical movements are well stated, though not always new; but whether they are wholly justified by the ancient mysticism into which his doctrine leads us is doubtful. It is an intensely interesting discussion, however, vigorous and earnest in style, and one to which the present writer feels much indebted.

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VISION.

Effect on the Persistence of Vision of Exposing the Eye to Light of Various Wave-Lengths. FRANK ALLEN. Physical Review, XI. (5), 1900.

The work that has been done from time to time at Cornell University, under the direction of Professor Edward L. Nichols, on the persistence of vision is of extreme interest. By utilizing the different means for attaining accuracy that are available in a well-equipped physical laboratory at the present time, results were gotten already in 1884 by Professor Nichols himself, and again by Ferry in 1892, which much exceed apparently in smallness of probable error anything that the psychologist has been in the habit of looking for. The method employed was essentially that of flicker photometry, comparison being made in this instance not between two different lights but between alternate light and darkness as regards the physical conditions, or, between alternate light-effect and its persistence as regards the physiological conditions; that is to say, spectral light was alternately let on and cut off by means of the sectored disc first employed by Aubert (and named by him the episkotister), and the absence of any flicker was taken as indicating that no perceptible falling off had taken place in the volume of the excitation produced in the visual apparatus during the interval when light was cut off by the filled-in portion of the disc. (It will be seen, therefore, that it is not persistence of vision, but *undiminished* persistence of vision that is in question.) This method is in frequent use, of course, in laboratories of real research; the source of the improvement as it is carried out at Cornell seems to be the greater accuracy with which the velocity of the rotating disc is measured. The disc was made to rotate by means of an electric motor, the speed of which was recorded electrically on paper placed on the cylinder of a chronograph; "so constant were the different adjustments of the apparatus, the intensity of the light, and the sensitiveness of the eye, that observations taken at different times, and even on different days, differed from the mean value by less than two per cent."

There is much that invites discussion in this article, but as further work is still going on in the subject it will be better to wait until fuller results have been obtained. Certain points may be mentioned, however, with a view to suggesting some possible changes in method. (1) Instead of observing a narrow band of color, it would probably be better to adopt König's plan of substituting a simple diaphragm for

the whole eye-piece of the spectrometer; by this means a large surface, instead of a narrow band, of homogeneous light (*viz.*, the face of the last prism) is presented to the eye, and the sources of error that lie in an extreme upsetting of the state of equilibrium of the retina are avoided. (2) Still more is it desirable in fatiguing the eye with certain colors that a larger surface should be affected; if only a narrow band—two millimetres wide—of color is looked at, it must be a very difficult feat to keep the light that impinges next exactly upon the portion of the retina already affected. (3) The argument by which the writer would show that blue, for instance, is not a simple color is plainly not rigorous. After fatigue by blue, it is not blue, he finds, but the adjacent colors, violet and green, that have their period of undiminished persistence lengthened (that have, that is to say, become of diminished brightness); and he infers from this that blue is not a simple sensation, but a compound of violet and green. But there are several objections to the thesis that the effect of fatiguing by a mixed color is to fatigue the element of the mixture disjunctively and to leave the mixture itself unaffected. In the first place, why was not this curious proposition tested directly? Nothing would have been easier, if it be true, than to show that in the case of fatiguing by orange and purple, or yellow-green, or blue-green (colors which are accepted by all theories as mixtures) the orange or the purple or the yellow-green or the blue-green showed, itself, no trace of fatigue, but was only a bright spot between the two exhausted elements of which, in the given case, it was composed. But again, the effect of white light on the retina shows conclusively that the author is amiss in the explanation which he has hit upon of his phenomenon. He gives the table in which are summed up his results concerning the percentage of increase in the (undiminished) duration of color impressions when the eye has first been fatigued by exposure to various colors:

Region Affected.	Color of the Fatigue Stimulus.					
	Red.	Green.	Violet.	Blue.	Yellow.	White.
Red	8%	0%	0%	0%	10%	9%
Yellow.	0	0	0	0	0	5
Green	0	12	0	10	12	13
Blue.	0	0	0	0	0	13
Violet	0	0	27	16	0	12

It certainly looks like a curious fact that while fatigue by red, green, or violet produces an effect upon each of those colors, respectively, and upon no other color, fatigue by blue produces no effect upon

blue, but only upon green and violet. But if this is because blue is not fundamental but a mixture, and if when a mixture is the cause of the fatigue the mixture itself does not suffer from the fatigue, how shall we explain what takes place when the source of the fatigue is white light? White is, physiologically, upon the present hypothesis, nothing but red and green and violet. We have just been told that green and violet when combined into blue have no effect whatever in the way of prolonging the duration of the blue light sensation (but only that of the green and the violet); what then is the cause of the fact that, when *white* light is the fatiguing agent, blue is affected just as much as either green or violet? It cannot be the green or the violet that does this (for neither of them affects anything but violet or green); it cannot be the red (for that affects only red); there is, in fact, no way of explaining it, and it can only be that the whole conception is wrong, and that, whatever may be the cause of the results described, it is not this (which would be anyway very improbable)—that green and violet, while they operate together to give the sensation blue, do not operate together to cause fatigue in blue.

There are other points in this paper which arouse consideration (to make use of a useful German phrase). The normal curve of persistence is reprinted, for the sake of comparison, with each figure of a fatigue curve; but it appears in two different forms—usually it shows a minimum persistence of .0120 seconds (and this is what is given in the table of values for the normal curve, p. 264), but in the diagrams exhibiting fatigue for violet and for blue this whole curve is transposed, and shows a minimum of about .0105 sec. There is doubtless some simple explanation for this, but none is given; and with a method which is said to show a variation of only two per cent. in observations taken on different days, to come suddenly upon a discrepancy of twelve per cent. is confusing. The difference makes, of course, a great change in the character of the blue fatigue curve, as exhibited in the diagrams.[1]

Again, the wave-length chosen for a light to cause blue-fatigue was $\lambda = .430\,\mu$, but this is already, according to the best observers, violet, and, in fact, a violet than which there is none redder in the spectrum; $.430\,\mu$ is just the limit beyond which no color change is perceptible.

[1] Mr. Allen explains, in a note which I have received from him since this was written, that the discrepancy here referred to is due to an unavoidable change in the brilliancy of the acetylene light used. The sentence which I have quoted at the end of the first paragraph above would, therefore, seem to need a slight modification. And it still seems singular that the light should appear at just two different degrees of brilliancy, and no others.

Both Hering and König are agreed (an instance of good fortune) that blue is $\lambda = .470\,\mu$. If they are right, then light of $\lambda = .430\,\mu$ ought to cause additional persistence, upon the assumption of this paper, in violet only, instead of in violet and green.

To conclude, this paper presents facts which (if they are confirmed by additional observers) will certainly prove to be of the very greatest interest; that there is any ground for the way in which they are here interpreted seems to be, however, questionable in the extreme.

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PAIN-SENSE.

Undersökningar öfver smärtsinnet. (Investigations on the Pain-sense.) By SYDNEY ALRUTZ. Upsala universitets årsskrift, 1901. Upsala, Akademiska Bokhandeln. Pp. 136. 2 kr. 50 öre.

The work is historical, critical and experimental. It places Dr. Alrutz in line with Blix, Goldscheider and von Frey. The monograph would be welcomed in an English edition as the best recent statement on this subject. Since it is accessible to only a small number of readers in its present form we shall here give a brief résumé instead of attempting a critical review. We shall omit the purely historical matter and the details about technique, and mention only some criticisms, positions taken in disputed questions, and new facts observed, although it is difficult to do justice to the work by this method of gleaning.

Chap. I. Pressure Spots.—These are studied for the purpose of determining their relation to the pain spots. For stimuli he uses a graduated series of Thunberg's glass hairs. The experiments show that in the hair-covered skin the pressure spots always lie to the windward of the hairs; that in its remaining area between the hairs no pressure spots could be demonstrated; that in the hairless areas the pressure spots exist and are more numerous and difficult to differentiate, but the sensations elicited from them are of the same quality as in those elicited from the hairy areas; that the withdrawing of the pressing point may call forth the same kind of pressure sensation as the applying of the point calls forth; that pain sensations are obtained from the pressure spots only when the pressure is great enough to irritate adjacent pain spots; and that likewise heat and cold sensations cannot be obtained by stimulating a pressure spot only.

Chap. II.–IV. Pain Spots: The Double Pain Sensation.—The latter title refers to Goldscheider's 'secondary pressure.' The experi-

ments show that both immediate (primary) and delayed (secondary) pain sensations may be produced by a point stimulus; that both cannot be obtained from each and every point, and that the primary sensation has a pricking, point-like character, while the secondary is of an itching and irradiating nature. In different spots one may produce, by pricking, only secondary pain sensations, or only primary pain sensations, or both, or may fail to produce any pain sensation at all. It is impossible to count them exactly, but the author thinks that von Frey's estimate of the number of pain spots is somewhat too low. Both pressure spots and cold spots were found capable of giving the secondary pain sensation, but neither would give the primary. A topographical study of the skin shows that the secondary pain (Goldscheider's pressure) sensation can be obtained in certain areas, *e. g.*, on the arm and the abdomen, and that it differs in intensity for different areas, and that it cannot be obtained in others, *e. g.*, on the tip of the nose and in the middle of the cheek. In general, the possibility of obtaining the secondary sensation varies topographically about the same for thermal as for mechanical stimuli. The secondary pain sensations readily call forth reflexes.

"There exist spots in the skin which, upon stimulation with a point, give pain alone, or rather a pricking sensation. * * * Neither the primary nor the secondary pain sensations have anything to do with pressure sensations. * * * That which specially characterizes the secondary sensation is that it has, at least in most parts of the skin, the character of pure itches when caused by a weak mechanical stimulation with a point. * * * The skin spots for the primary, pricking pain sensations do not coincide with the spots that most readily and clearly give the secondary or more or less itching sensations. * * * I believe that there is a qualitative difference between the primary and the secondary pain sensations."

Chap. V. Pain, Pain Nerves and Feeling Tones.—The historical résumé in this chapter is most excellent. Only a few of the criticisms can be noted. He opposes Goldscheider's view that there is a resemblance between pressure and pain. The theory of Nichols in regard to specific end organs is rejected. Marshall's criticism of the sensation hypothesis can be valid only on the assumption that it is directed against the view that would make sensations of the feeling tones. The poverty of the English language in possessing only the single word 'pain' to denote widely separated facts of consciousness is the cause of endless confusion in reasoning. Strong's theory of the coördinated sensations is criticised. The author's view is that, "in

case of very strong pain stimuli, the pain sensations coming from the end organs of pain are so intense and unbearable that the pressure and temperature sensations that perchance occur concomitantly fall into the background of consciousness, or are even suppressed." Among several other views of von Frey, his classification of pain as both sensation and feeling is rejected. In the debate between Richet and Fredericq the author sides with the latter.

On analgesia and hyperæsthesia for different stimuli on the same cutaneous area, the author cites two cases of clinical observation which lead him to think that the conditions under which these phenomena occur are very complicated, and of course pathological, and that Strong's cases on this point need to be verified before we dare build any theory of specific energies upon them.

Chap. VI. Sensations of Tickling and Itching.—The historical statement is followed by some experiments. A single stimulation of a single point may bring about these sensations. The repetition of the contact may simply cause a summation of infra-liminal stimuli. There is no close resemblance between the tickling sensations and sensation of touch, and it is difficult to observe any resemblance between tickling sensations and sensations of pain. On the contrary the itching sensations both resemble the pain sensations and have transition forms to the pricking pain sensation. Evidences are given to support the theory that the itching sensations have a distinct quality, and that there are distinct end organs for the mediation of this quality. The difference between itching and tickling is simply a difference in degree.

Chap. VII. Pain Qualities.—The historic review is followed by an explanation of the 'paradoxical cold sensation.' The author demonstrated this phenomenon at the meeting of the Paris International Congress of Psychology. Some time after a cold stimulus has been applied and removed a secondary sensation of cold may arise and be more intense than the primary. It is caused by the warming from within. Experiments show that Rollett's conclusion that chloroform and ether cannot produce real temperature sensations is false. The heightened sensibility in places stimulated with menthol may be explained by the heightening of irritability caused by the action of the menthol on the end organs of temperature and pain. The pain sensations produced by heat are identical with those produced by cold. Hence if we distinguish between heat-pain and cold-pain we thereby denote the elements of complexes, *i. e.*, addition of temperature to the quality of pain. The pain produced by mechanical stimulation is of the same quality as that produced by cold. There is a difference

between dull and aching pains. The dull pain comes from the deeper-lying organs. The difference between pricking and aching pain is primarily a difference in the spatial attribute of the pain quality. As stated above, itching is a distinct quality of sensation, but between it and tickling there is only a difference of degree. The termini of the pain, cold, and heat nerves lie in the order mentioned inward from the surface. The cause of differences in the elementary quality of pain is not to be found in the difference in the kind of stimuli. The same means may produce different qualities and the same quality may be elicited by entirely different stimuli. Different qualities undoubtedly depend upon different end organs, both peripheral and central. A bibliography is given.

This monograph presents an extreme view, as is seen in the above. But, for determining the conditions of the end organs and the elementary sensations of the skin, this tendency is needed in the investigation in order to search out the facts. The opposing extreme simply slurs matters over by speaking of 'nerfs tactiles' and 'feeling pain.' The skin is yet a mysterious and unknown territory. The explorers tell us strange and conflicting stories. This is chiefly on account of the complexity of the phenomena.

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EQUILIBRIUM AND VERTIGO.

Le Vertige: Etude physiopathologique de la fonction d'orientation et d'équilibre. DR. GRASSET. Revue Philosophique, Vol. LI., 225–251, 385–402, March and April, 1901.

The aim of this article being an examination of the physiological processes concerned in a certain abnormal condition, rather than a discussion of their bearing on the psychology of space, the author at the outset agrees to pass over all questions of space perception, the origin of the space notion, and the relative importance of the several senses in determining this notion. He proposes to discuss simply the particular nervous apparatus concerned in orientation and the maintenance of equilibrium, its physiology and pathology. As he points out, the former function (orientation) is primarily sensory, though its apparatus includes afferent nerve connections, while the latter (equilibrium) is primarily motor, its apparatus including, however, sensory connections which serve to determine the position of the parts of the body concerned in maintaining the equilibrium. The two act together as a single apparatus or organ by means of a series of centers, some of

which are automatic or reflex, while others are voluntary. Dr. Grasset, it should be noted, limits the term 'equilibrium' (équilibre) to the motor function exclusively, and uses the term 'equilibration' (équilibration) to denote the combined sensori-motor function.

A large portion of the article is devoted to the normal workings of the function of equilibration. The discussion of the nervous apparatus concerned in this process is perhaps the most interesting and valuable part of the study; the psychologist, at least, will deem it so. We shall give a brief summary of the conclusions reached in this part, omitting the neurological minutiæ.

The author first discusses the various centripetal courses, *i. e.*, those which contribute to the sensation-complex of orientation. These he limits (in man) to the kinæsthetic, tactile, labyrinthine, and optic courses. While distinguishing carefully between the general kinæsthetic sense and general tactile sensibility, he thinks the two should be classed together for purposes such as the present discussion, since they coöperate in furnishing data regarding the position of the body and its members, the state of contraction of various muscles, as well as the form, weight, distance, etc., of nearby objects. The kinæsthetic apparatus in the labyrinth of the ear is similarly distinguished from the auditory (sensory) apparatus adjacent to it, and is regarded by the author as prior to the latter in origin. Each, however, contributes data for the orientation of the head. The orientation function of vision is likewise dual: by vision proper we determine the form, size, and distance of objects; but the motor apparatus of accommodation, eye movement, etc., aid considerably in these determinations. The centripetal courses of orientation are thus seen to belong to two main categories: (1) Extrinsic courses: those which transmit impressions obtained from outer stimuli—notably touch, hearing, and vision. (2) Intrinsic courses, which transmit impressions obtained from inner stimuli; these include the general kinæsthetic apparatus, the special kinæsthetic apparatus in the ear, and the special kinæsthetic apparatus connected with the eye.

The centrifugal courses concerned in the equilibrium function (in the narrower sense) comprise all nerves which lead to the muscles, either for contraction or for relaxation, whether voluntary or automatic. No special classification of these is given.

The centers and central courses are discussed at considerable length. The author regards it as proved that the cerebellum has all the elements necessary to constitute it an independent center for the function of equilibration; it may therefore be considered a reflex cen-

ter of that function. But in the cortex and basal ganglia several centers are found which exert control over the function, viz., the centers of the centripetal courses already mentioned; by means of these it becomes a voluntary function. These latter centers supplement one another in the work of control, and in case of functional disturbance in any one of them the others may take its place. The bearing of this on the pathological state of vertigo is apparent; thus, in sea-sickness, where the disturbance is partly visual, closing the eyes is beneficial; in gastric vertigo, on the contrary, opening the eyes affords some relief. The author proceeds to discuss at some length the relation of these higher centers to one another, and to the subcortical centers of equilibration.

Turning now to the problem of vertigo, we find Dr. Grasset insisting on its essentially sensory character. This is set forth in the very beginning of the article (from which portion we quote) and reiterated later.

"Vertigo," he says, "is a *sensation*, that is to say, it is a *subjective phenomenon of consciousness*. It may have objective causes and consequences; but essentially it is subjective and conscious. * * * In the second place, it is a *deceptive* (fausse) sensation. * * * [It may be] either hallucination or illusion. * * * This deceptive sensation, which constitutes the primary element of vertigo, is a sensation of *displacement* (*rotation*) *of the objects about us, or of our own displacement* ('*entrainement*') *in relation to the surrounding objects.* * * * There is [however] in vertigo another constituent element besides that which we have already analyzed. * * * [This second element is] *the sensation of the loss of equilibrium.*" (Pp. 226–233; the italics are in the original.) These are the leading points in Dr. Grasset's statement, which is supported by many citations of evidence and interspersed with arguments against other current views—notably that of M. Pierre Bonnier, who includes the sensation of *reeling*, as well as the correlate motor phenomena, under the head of vertigo. The first impulse of the present reviewer was to exclaim against Dr. Grasset's disregard of these motor elements, which obviously stand in close relation to vertigo; his second thought was that this, being largely a question of nomenclature, might well be left to convenience or analogy for settlement. Thus, the special sense organs have motor connections, but the sensation side is so prominent that they are classed as sensory apparatus; *per contra*, in pathological phases of these same functions sensory and motor elements may be equally prominent, as in the case of strabism. In the case of vertigo, the motor disturbance may be re-

garded as a stimulus of the specific sensation. The present tendency in psychology is to trace out the sensori-motor connection where possible, and correlate the two sides in the discussion of any particular function. The real criticism that we have to make on Dr. Grasset's position, therefore, is that while the sensation under discussion is an important element in disorientation and disequilibration, still the total sensori-motor disorder includes something more than this, and all elements should be included in any general treatment. This is perhaps chiefly a criticism of the heading of the article; the subject matter, the reviewer thinks, would justify a broader title to indicate its actual scope.

As for the general question of terminology, it seems important to distinguish between the sensation and the sensori-motor disorder. In English we have the word *nausea* to indicate one prominent element in the sensation-complex; we have, further, the two words *vertigo* and *dizziness*, one of which might be used to denote the sensori-motor disorder of which this sensation-complex is the conscious manifestation. A complete differentiation of these three words would tend to clear the analysis of the phenomena in point.

In concluding the article, Dr. Grasset gives a summary of disorders of orientation and equilibrium, and discusses the several types which he finds. His scheme may be indicated briefly as follows: *I. Subjective symptoms:* (1) Anæsthesia; diminution or loss of sensations which give rise to orientation (kinanæsthesia, etc.). (2) Hyperæsthesia; abnormal increase of these sensations (abnormal fatigue, cramps, etc.). (3) Paræsthesia of orientation alone (disorientation). (4) Paræsthesia of orientation and equilibrium together (vertigo). *II. Objective symptoms:* (5) Akinesia (paralytic astasia-abasia). (6) Hyperkinesia (movements of 'entraînement' and propulsion). (7) Parakinesia with irregular contractions, in walking or other movements (ataxia and incoördination). (8) The same, in state of rest on the part of the subject (chorea, etc.). (9) Parakinesia with tremors, in walking, etc. (multiple sclerosis). (10) The same, in state of rest (paralysis agitans).

H. C. W.

EXPERIMENTAL.

1. *Les méthodes de l'esthétique expérimentale. Formes et couleurs.* J. Larguier des Bancels. L'Année Psychologique, 6e Année, 1899, 145-190.
2. *Recherches anthropométriques sur 223 garçons anormaux âgés de 8 à 23 ans.* Th. Simon. Ibid., 191-247.

3. *Attention et adaptation.* A. BINET. Ibid., 248–404.

4. *Recherches sur la sensibilité tactile pendant l'état de distraction.* A. BINET. Ibid., 405–440.

5. *Expériences de suggestion sur des débiles.* TH. SIMON. Ibid., 441–484.

6. *Formation des voyelles.* DR. MARAGE. Ibid., 485–492.

1. This article reviews the various efforts made to solve the chief problems of æsthetics, dwelling principally upon an analysis of the labors of Fechner, with the aim of pronouncing upon the validity of the various methods which have been employed in more recent experimentation. The author concludes that experimental methods can be applied to only a small number of æsthetical problems, and that the field must continue to remain largely in the hands of 'general psychology.' The observation of the relatively simple phenomena of an æsthetical quality is regarded as the smaller task of æsthetics.

2. This memoir records original measurements made upon degenerates, defectives, and idiots comprising the colony at Vaucluse. The object of this study is to find, as nearly as possible, the correlation there may be between physical development and intellectual capacity, which is affirmed to be directly traceable. The facts of the study were derived from the following measurements: The height, the chest, breadth of shoulders, maximum circumference of the head, the weight, and the stretch of the arms. The special studies, worked out with great detail, include correlation of different measurements, a comparison, by ages, of the measurements of these children with the results of studies of normal children made by some twenty observers, from Schmidt to Hrdlicka, and also a comparison of the extreme grades of intellectual capacity displayed by members of the colony. As to this last point, it is found that "it is less an alteration of the proportions of the various parts of the body which distinguishes the idiot or the imbecile from the weak-minded, than the absence of his development considered in its totality."

3. This is the most interesting and most important memoir in the *Année.* In aiming to organize methods for estimating or measuring the force of voluntary attention, Binet discovers a complement to the processes of attention which is indicated by the second term in the title; thus he offers a new point of view from which to study voluntary attention. B. selected his subjects from a class of 32 children in a public primary school in Paris, arranging them in two groups, one including the five 'most intelligent,' and the other the six 'least

intelligent' of the whole class. 'Natural intelligence,' rather than diligence or industry in school, as judged by several teachers, was taken as the basis of the selection of these children.

B. aimed to demonstrate the differentiation of the two groups by the application of well-known experimental tests of the following types: Tactile sensibility (back of the hand), simple and choice reaction time, counting dots, perception of increase and decrease in stimulation, counting rhythmic sounds, copying texts, series of numbers, phrases, nonsense texts, and designs, memory of letters, numbers, etc., accuracy in proof-reading and marking letters, simultaneous additions, and speed in reading figures, etc., etc. The tests comprised difficult and unattractive intellectual work not beyond the range of the children, whose ages varied from nine and a half to thirteen years.

B.'s criteria for selecting the tests in his preliminaries were as follows: Those tests which showed rather equal averages from the two groups were regarded as bad; those tests whose results demonstrated the intelligent group to be the better, were regarded as good. The degree of attention is measured by the number of errors committed by the subjects in any given test. B. is aware that the mental functions involved in each test are numerous and complex, but he does not attempt to analyze them. In this study he takes account only of the results. The errors above mentioned are regarded as the involuntary defects of attention, as dependent solely upon the mental constitution of the subject (p. 395).

The tests for perception of changes in the rapidity of the strokes of a metronome, rapid reading (by shutter exposure), and reaction-time constitute a group which does not present results corresponding to the grade of intelligence. Quickness, also, is found to be independent of intelligence. The remaining tests comprise a group whose results differentiate the two classes of children. The following particular features of the tests may be mentioned. The intelligent group had a much finer tactile sensibility at the first trial, but their improvement in later trials was less than that of the unintelligent group. This test was good for a classification of the subjects. Counting dots revealed no great differences, and does not seem to be a good test for a classification. The first trial at counting rythmic sounds is good for classification, the intelligent children being much better, but the differences diminish after the first trial. Copying numbers is a good test for showing individual differentiation, while memory of numbers, analysis of a design, and simultaneous addition are excellent tests to exhibit group differentia-

tion. Correction of proofs differentiates the groups and classifies the individuals at the same time. B. did not attempt to correlate these tests in the present study.

B. views attention as a process of mental adaptation to a state that is new to us. In repeating tests he noticed less inequality between the two groups of children, and this was the most disturbing factor appearing in his problem. The reduction of group-differentiation he found to be due to the relative quickness of the adaptation of the pupils. Adaptation in the intelligent children is different from that in the unintelligent. The intelligent pupil adapts himself in the first trial better and quicker; the unintelligent pupil is slower the first time, but improves in adaptation in successive repetitions, thus reducing the group-difference. B.'s most important suggestion is that we should not lose sight of this process of adaptation, as it might vitiate results considered to be well established. This memoir is an important contribution to both individual and pedagogical psychology.

4. In making various efforts to find the difference in tactile sensibility between 'intelligent' and 'unintelligent' subjects, B. was led to the study here reported. The subjects were two girls and a young man. Compass contacts on the back of the left hand were used. Distraction of attention was brought about by giving tasks in addition of numbers, the subject repeating each progressive step aloud, the contacts being made during the search for the sums. B. finds that distraction cannot be produced uniformly, the question of the individual being of prime importance. A state of distraction was manifested in some cases by a verbal automatism which consisted in generalizing the answers. The percentage of answers, 'one point' and 'two points,' does not express a special state of sensitiveness under these conditions, nor does it serve to determine the threshold of sensitivity. The subject whose mind is distracted does not remember the order in which the contacts took place; and this uncertainty of memory, compared with the memory left by a séance when the subject paid attention, proves that distraction could diminish his consciousness.

5. Simon here reports the results of tests conducted by himself on some twenty-seven of the children studied in No. 2, at Vaucluse. He employed the methods of Binet in the latter's studies on the suggestibility of normal school children (see B.'s volume, *Suggestibilité*). There were five groups of tests; estimating the length of lines, the weight of cubes, length of lines exposed on a revolving disk, remembering groups of objects exposed on a card, and performing rhythmical movements. In the first two and the last tests, the suggestion was induced

by objective increase in stimulation, while the experimenter sought to produce suggestion verbally in the remaining tests. This study is admittedly meagre and imperfect, owing to the lack of subjects (varying from twenty-four to eight) and the vagueness of the results, which cannot be classified in any satisfactory manner. Automatism seemed less noticeable in the perception of weights and the production of movement. S. arranges these subjects into five types: The wholly imbecile, which is beyond suggestibility; those of firm judgment which prevents suggestion; those of medium suggestibility, who are most numerous; the automatic; and the bizarre type, which is very irregular and indefinite. In comparing his results with those derived by Binet from normal school children, S. finds these types analogous to those of B.'s excepting that the bizarre is not noted, while the automatic type is found more frequently among the normal children.

Weak-minded and normal children seem to behave about the same; though the former seem less succeptible to suggestion, which is, in part, ascribed to the emotional character of the reactions of the latter.

6. This account of a study from the physiological laboratory of the Sorbonne, after reviewing the three theories of Helmholtz, Hermann, and Guillemin, which are set aside, offers the following explanation of vowel-formation: "Vowels are due to an intermittent aerolaryngean vibration, reinforced by the buccal cavity and producing *ou*, *o*, *a*, *é*, *i*, when the latter is in unison with the sum of the vibrations; transformed by it (the buccal cavity) and forming the other vowels when the unison does not exist; the number of intermissions gives the fundamental note on which the vowel is uttered. A whispered vowel is produced when the buccal cavity functions alone; the vowel is sung when the larynx functions alone; when the buccal cavity and the larynx function simultaneously, the vowel is spoken."

EDWARD FRANKLIN BUCHNER.

Avons-nous des sensations spécifiques de position des membres? ED. CLAPARÈDE. L'Année Psychologique, 7e Année, 1900. Pp. 249–263.

Claparède here vigorously reaffirms his negative answer to this title-question, by reviewing briefly the logical and psychological arguments in favor of the two possible answers; by referring to a few results which he has derived from tests on movements; and especially by a critical discussion and final rejection of the views of M. P. Bonnier, published in the latter's *l'Orientation* (1900), which maintain an affirmative answer to the question. Position cannot be felt immedi-

ately. It is a complex phenomenon, involving the intermediation of an association of images, and appearing as the result of 'inference,' the term with which C. replaces the word 'judgment,' employed in his earlier expositions. He thus indicates that our ideas of position as such are not due to mere sensations, nor to an active, conscious operation or process. Position is only a state of relation between diverse data presented by the pressure and tensions experienced in the portions of an arm, *e. g.*, which has moved. The determination of position is only a particular case of that psychical operation known as the localization of parts of the body, which in turn is regarded as totally dependent upon the fact that the various portions of the body offer surfaces possessing different degrees of sensitivity, whence the 'local color' which is a quality in tactile and muscular sensations. Thus position is an affair of perception, rather than the content of sensations locally derived; and it usually involves an escort of visual images which are called forth by muscular-tactile impressions. C. insists that introspective and experimental data can be interpreted only to the conclusion that 'sensations' of movement and of position must be regarded as qualitatively different.

One half of the article is devoted to an examination of the materialistic views of Bonnier, who regards the alleged 'sens des attitudes segmentaires' as the foundation of all our mental life. C.'s rather caustic strictures are properly placed, saving us, as they would if widely known, from a perennial pseudo-psychology which says that "tactile orientation results immediately from the topographical distribution of images in our [cortical] centers"!

EDWARD FRANKLIN BUCHNER.

The Synthetic Experiment. I. M. BENTLEY. Amer. Jour. of Psychol., XII., 2. Pp. 405–425.

A synthetic experiment is one where a given complex conscious state is made to result from the simultaneous production of simpler states. Dr. Bentley's article contains, first, a discussion of the theoretical significance of such experiments, and second, an account of methods used in the experimental synthesis of the tactual perception of liquidity. In the first part, he indicates the value of synthesis as a test of analysis, and notes that to be sure of the validity of a synthesis we must know (1) that no extraneous suggestion has produced the experience and (2) that the experience is produced only when all its components are present. He points out the difference between a synthetized perception and an illusion, a distinction that rests largely on

extrinsic circumstances, such as the way in which the experience is produced, and its congruity with other experiences. The special conditions and difficulties of the synthetic experiment in various departments of mental life are referred to, as for instance, the difficulty of synthesis in the case of feeling because of the absence of voluntary control over organic sensations, and the import of artificial conditions in the reaction experiment as a synthesis of action. The most significant portion of this theoretical discussion is the author's careful statement that successful synthesis cannot after all prove the exhaustiveness of the corresponding analysis; it shows only that if u put certain simpler processes together the required complex process will result, not that the complex process is merely the sum of the simpler ones. It does not prove, in other words, that the complex perception may not contain something which cannot be isolated as a component, but results from the togetherness of the other components.

The author's experiments in the synthesis of liquidity are highly suggestive in the matter of method. He preceded them by requiring his subjects to analyze the perception of various liquids: it would seem better, by the way, in order to avoid every possibility of extraneous suggestion, not to have this preliminary analysis made by the subjects who are to serve later in the synthetic work. The subjects had their eyes blindfolded and nostrils stopped, and the introspection was carried out for perception of simple contact with the tip of the finger, dipping the finger in, and pulling it out. The results for three typical liquids, mercury, benzine and water, indicate that pressure and temperature are the chief components, and that wetness is not itself perceptible. The synthetic part of the experiment endeavored to produce the requisite conditions of temperature and pressure. Dipping the finger into a thin sheet of rubber stretched over a beaker failed to give the perception of liquidity; flour in a funnel, with the finger inserted into it through a small brass ring to simulate the liquid boundary, was almost completely successful; while the full perception of liquidity resulted from inserting the finger into a rubber sheath formed by putting a small weight on the middle of a thin rubber sheet stretched over a beaker of *cold* water, the rubber being thus pulled down into a dry pocket closely surrounded and pressed upon by the water. Here the temperature and pressure conditions were exactly right, but wetness was quite ruled out, thus confirming the evidence of analysis that it is not a component part of the perception.

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On some Minor Psychological Interferences; a Study of Misspelling and related Mistakes. T. LE MARCHANT DOUSE. Mind, IX., 85–93. January, 1900.

This is a brief study of *lapsus calami* observed in the written 'answers of candidates at a certain university examination.' They are classified under five heads: (1) Prolēpsis, or 'Assimilation from ahead' (*e. g.*, in*dro*duce, mo*rder*n), (2) Metapēdēsis, or 'Overleaping' (*e. g.*, super*sti*on for super*stiti*on), (3) Metallagē, or 'Cross Compensation' (*e. g.*, Pa*dog*a for Pa*goda*), (4) Opisthomimēsis, or 'Assimilation from the rear' (*e. g.*, Hou*sehou*ld, Evi*dendence*), (5) 'Contamination,' in the philologist's sense (*e. g.*, 'T*ue*tonic' was written for 'T*eu*tonic' through the unconscious influence of the word 'Tuesday').

This classification may be compared with that of the present writer in his 'Study of Lapses,' in which (1) is treated under 'Anticipatory Substitution' or 'Insertion,' (2) under 'Anticipatory Elision,' (3) under 'Exchange and Inversion,' (4) under 'Persistence,' and (5) under 'Conflict and Coalescence.' As was there pointed out (and this is confirmed by the article here reviewed), the greatest psychological interest centers about the phenomena of conflict and coalescence or 'Contamination,' since in such errors we have the actual process of disintegration and reconstruction going on.

Attention is called to the fact that the mind tends to throw the material presented to it, no matter how inchoate, into some form which will 'carry a meaning.' This is seen in the 'Popular Etymology' which, for example, transforms 'asparagus' into 'sparrow-grass.' The author also calls attention to the occurrence of *lapsus linguæ* conforming to his five types, but he does not develop this side of the subject; he contents himself with a few sporadic instances by way of illustration. This is an interesting subject and one could wish that the author had expanded his treatment on the psychological side.

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VASSAR COLLEGE.

MENTAL TESTS.

The Correlation of Mental and Physical Tests. CLARK WISSLER. Monograph Supplement No. 16 to the PSYCHOLOGICAL REVIEW. New York, The Macmillan Co. June, 1901. Pp. 62.

This monograph concerns itself with a statement of the methods and conclusions pertaining to an attempt at correlating a series of psychological, academic and physical tests. The data had been in process of accumulation for several years by the Department of Psychology at

Columbia University. The following tests were made yearly upon sixty to seventy freshmen of Columbia College and repeated upon those who remained to the end of the senior year: Length and breadth of head, strength of hands, fatigue, eyesight, color vision, hearing, perception of pitch, perception of weight, color preference, reaction-time, rate of perception, naming colors (rate of reading), rate of movement, accuracy of movement, perception of time, association time, imagery, auditory memory, visual memory, logical memory and retrospective memory. Records of stature, weight, lung capacity, etc., together with data concerning parentage, personal habits and health of the men tested were taken by the Director of the College Gymnasium. In the examination of the data obtained from the above the author employed the method of correlation developed by Karl Pearson, from which the relative degrees of functional relations holding between the various pairs of tests can be estimated with considerable precision. The data for some 250 freshmen were treated under three main divisions: Psychological tests, academic tests and physical tests. The former were chiefly tests of quickness and accuracy. Correlating the results for each test of this group with the others showed the relative rank of the individual in the group to be little more than a mere chance relation, or, in other terms, there is no evidence of important functional relations between the activities employed. This leads the author to say that these tests must be measures of special abilities. The result was the same in correlating the memory tests. The college standing furnished the data for the academic tests, and correlation within this group revealed an agreement of rank between the different subjects of study to a degree equal to that holding between the height and weight of the same students. The physical tests were found to correlate with each other to a considerable degree, as stature with weight, strength of hand with lung capacity, etc.

When the author attempted to correlate the psychological tests with the academic and then in turn with the physical, the relation was little more than that of chance. This was also true of the relation between the academic and the physical tests. The general conclusions of the research are: (1) That the laboratory mental tests show little intercorrelation in the case of college students; (2) that the physical tests show a general tendency to correlate among themselves, but not with the mental tests; (3) that the markings of students in college classes correlate with themselves to a considerable degree, but not with the tests made in the laboratory.

THE AUTHOR.

PSYCHOLOGICAL CONGRESS.

IV^e Congrès International de Psychologie, Paris, 1900; Compte Rendu des Séances et Texte des Mémoires. DR. PIERRE JANET, Secr. gen'l du Congrès. Paris, Felix Alcan. 1901.

Some of the papers are printed in extenso, others in epitome. It is possible to give here only terse amplifications of the titles. The following embraces the papers that might be grouped under the head of Philosophical Psychology.[1]

A. 4e SÉANCE GÉNÉRAL.—ÉTUDES PHILOSOPHIQUES SUR LA PSYCHOLOGIE.

Sieben Rätzel der Psyche. Dr. K. B.-R. AARS, Christiana.—The following are simple and fundamental facts—problems inaccessible to analysis: psychic elements, simultaneous comparison, successive comparison, the state of expectation (futurity, causation), the process of identification, objective projection, and the existence of psychic beings other than self.

Note sur la conscience de l'effort intellectuel. Professor HENRI BERGSON, Paris.—(1) Every intellectual work consists in passing from a design or scheme to an image. (2) In every intellectual effort there is a strife on the meeting of multiple and analogous images which try to thrust themselves into one and the same design, some not filling it up entirely, others passing by until at last the coincidence of the image with the design is attained. This movement of all particular images gives us an impression *sui generis*, which ought to enter largely into the consciousness that we have of intellectual effort.

Psychologische Atomistik. Professor HUGO MÜNSTERBERG, Cambridge, Mass.—A hierarchy must develop in psychology, like that found among the natural sciences. This will begin with the purely theoretical, indivisible element; hence, psychological atomism. This will have nothing to do with the physical atom. Nor is sensation this ultimate element. We obtain the first suggestion of this *Urelement* in the principle of similarity. So long as sensations are similar they cannot be absolutely simple. The psychic *Urelemente* must be completely dissimilar. In physics the aim is to reach elements that shall be absolutely similar and devoid of quality. Psychology demands atomistic elements that differ only in quality, and are qualitatively incomparable. Herewith the distinction between the different senses

[1] Other papers will be noticed in these pages. See also the general report of the meeting in November, 1900, number of the REVIEW.—ED.

falls. The aim is now to build a system out of this chaos of postulated elements by arranging them in series which are not similar. The principle of assimilation will here be helpful. The speaker illustrated fully in the various sense departments how the process of assimilation makes it possible to construct a system of psychic elements. The aim was merely to show the possibility.

La douleur. Professor VLADIMIR TSCHISCH, Dorpat, Russia. —The speaker took exception to Richet's theory that pain is produced by strong stimuli and by all abnormal states. Chemical substances that transform living tissue into dead tissue produce pain. The same law may be adapted to chemical, electrical, and thermal stimuli. Such excitations as cannot kill, *e. g.*, a brilliant light or a disagreeable perfume, do not produce pain. Such stimuli as can kill, *e. g.*, venomous substances, mechanical and thermal stimuli, do produce pain—however, not all, *e. g.*, morphine, cocaine. All stimuli that are harmful to the individual produce disagreeable feelings; such stimuli as kill the individual produce disagreeable feelings; stimuli that kill living tissue, transforming it into dead tissue, produce pain. Pain may be regarded as a danger signal.

Terminologie psychologique. ED. CLAPARÈDE, Geneva.—There is urgent need of a rigorous scientific terminology in pyschology. The speaker requested that a committee of one for each of the four languages represented at this congress be appointed to formulate some elementary definitions and report same for discussion at the next meeting of the congress. Favorable action was taken on this by the appropriate committee.

B. SECTION II.—PSYCHOLOGIE INTROSPECTIVE DANS SES RAPPORTS AVEC LA PHILOSOPHIE. (5 Séances.)

La psychologie des sports. FRANÇOIS DA COSTA GUIMARAENS, Paris.—The attraction in sport lies in the excitement that it produces. This pleasurable excitement is simply the result of an increased activity of the vital functions. This increase of activity is due simply to a more active oxygenation. Therefore the attractiveness of sport is simply the excitation produced by oxygen. This paper led to considerable discussion on the topic.

Sur les théories herbartiennes et physiologiques du plaisir. PIERRE TISSERAND, Bourges.—The Herbartians admit two kinds of pleasure, physical (sensational) and moral (affective). To these they attribute different causes: to the first physiological and to the second psychological. This is wrong. We cannot separate moral pleasure

and physical pleasure; and every feeling has a physiological condition. The Herbartians make a false separation between soul and body and between sensation and feeling. Pleasure does not belong to the domain of mechanical necessity, nor to thought as a pure act; it is inseparable from life and spontaneity.

Sur les sentiments et les sensations et leurs différences fondamentales. Mme. MARIE DE MANACEINE, St. Petersburg.—Sensation and feeling have always been confused. Sensations may be divided into objective, *e. g.*, sight and hearing, and subjective, *e. g.*, pain, hunger, and thirst. The elements of time, space, and causality are realized in every sensation. Feelings obey only the principle of causality. Joy fills the body to the finger tips. A man under the domination of a feeling swears eternal faith or vengeance, as if the feeling were permanent. Sensations bring the body into the foreground of consciousness and tend to obscure the mental self. Thus cutaneous pain accentuates the consciousness of spatial relations of that part. Feelings tend to make us oblivious to the body, *e. g.*, in martyrdom. Excessive sensations may run into the quality of painfulness, but the affective states do not change quality with increasing intensity, though strong enough to kill. Sensations are aroused by physical stimuli: feelings are not. From this point of view sexual attraction lies primarily in a sensation. A classification of the feelings follows.

Péripatétisme et psychologie expérimentale. E. PEILLAUBE, Paris.—Modern experimental psychology is coming to adopt the peripatetic method. This is the objective method. The first chapter in psychology should be a discussion of life in all its aspects. This is the Aristotelian procedure.—In the discussion Professor Séailles held that the agreement between the peripatetic and the modern methods is only superficial. M. Peillaube made an elaborate defense.

Die verschiedenen Richtungen der Weltanschauung. EUGEN VON SCHMIDT, Freiburg in B.—Materialism is not philosophical. Spiritism is not scientific. Rationalism (spiritualism) is both scientific and philosophical. What is then life according to monistic rationalism? "It begins with the life-force becoming individual as Soul, which then strives gradually to build its body according to immanent laws, and to develop itself until the organism ends, *i. e.*, dies." The soul arises from the souls of the parents. It is not a gradual formation through the development of the sexual elements, for that would be a process in time. It comes into individual existence at the *point* of the first contact of the two sexual germs. The plant kingdom

represents the lowest stage of life—unconscious sensibility and striving; man the highest, and animals an intermediate stage. The modern doctrine of organic evolution is considered unreasonable. "With man a new kingdom is come to the earth, the kingdom of Spirit to which the ape does not belong . . . Spirit is reasoning power through which the reason becomes self-conscious or personal."—In reply to the question of Dr. Pavicié about the speaker's conception of the nature of spirit dissociated from body, the speaker replied: "The soul is a psychic force, an individual life-force, which, like every 'Naturkraft, is indestructible but loses its individuality in death and may return to the general life-force. It [the soul in which the world-reason has become personal] leaves all earthly behind and goes, as an immaterial point, . . . into a higher world, but retains the last result of its earthly development, which I have called memory's traces of the life on earth. . . . The eternal World-reason reaches personality in man, but it cannot satisfy itself with a mere earthly, human personality in the endless 'Weltall,' but develops through innumerable single worlds into innumerable personalities, each on a higher spiritual stage than the preceding."

The Psychological Impossibility of Scepticism as Shown in the History of Pyrrhonism. MARY MILLS PATRICK, Ph.D., Constantinople.—Pyrrhonism furnishes the best historical illustration of an attempt at absolute scepticism. Those men in the Pyrrhonic school who succeeded most nearly to attain to ἐποχή, or suspension of judgment, so completely failed in doing so as to prove the psychological impossibility of scepticism. They ran into a veiled dogmatism. For example, with Pyrrho, equilibrium of soul which may exist because of the impossibility of knowledge becomes a positive theory for the attainment of happiness. His life revealed this dogmatism. Scepticism pure and simple is not a philosophy that one could live. Aenesidemus and Sextus Empiricus are brought forward as further illustrations of the same fact.

Identité et continuité du moi. PAUL CARUS, Chicago.—We distinguish three elements which together form in our consciousness an organic whole: consciousness of our body as a unit, the continuity of our personal history, and the identification of our being with our aspirations and ideals. The consciousness of bodily unity is imposed by the necessity for its acting as a unit. It is not the substance but the form that persists. The true identity is experienced in the identity of our ideals. Individuality is the life of the body in time and space. Personality is the form of life, of thought, of feeling. Individuality

and personality are two aspects of the same reality. The individuality perishes; the personality persists.—In the discussion Professor Chatterjii, of Benares, said that this is no new idea. It is the fundamental doctrine of ancient Buddhism. Professor Buisson, of Paris, asked, What is form without matter?

La classification des états de conscience proposée par le Prof. Fr. Brentano et ses applications à l'analyse psychologique des jugements. Professor V. VAILATI, Italy.—The speaker developed an elaborate and artificial classification of judgments based largely on Brentano's theories.

The Psychology of Tickling. Professor JAMES SULLY, London.—Professor Sully's paper is printed in extenso in the report before us. The precise nature of tickling sensations is not yet fully understood. These sensations have a tendency to irradiate. They are allied to the organic sensations. All parts of the body are not equally susceptible to titillation. Unknown sense qualities may be involved. The feeling tone is complex: sometimes pleasant and sometimes predominatingly painful. Tickling elicits two distinct tendencies in movements: defensive movements and movements expressive of pleasure. The former generally occur in response to disagreeable sensations, though we must not forget the complexity of the process. The power to call forth the laughing reflex is not limited to the stimulation of end organs at certain depths or in any special area. Laughter is not merely the expression of the feeling tone of the sensory process. Laughter and the defensive movements may occur together. Self-titillation does not provoke laughter. The laughter is due partly to the assignment of meaning to the sensation. To this apperceptive process the following mental conditions are favorable: Indefinite expectancy, uncertainty, uneasiness, apprehensiveness, and above all a good disposition and playful attitude. Animals are ticklish. In infants it appears first near the end of the second month, and may, therefore, have psychic antecedents. It is an inherited reflex. Possibly it may have originated in the pleasurable experience of the lower animals in having insects picked from their hair. It may also be a trace of imitation of play at war or fighting. Hence its utility. The volume of the laughter may be accounted for by the swift recovery from momentary, half-developed fears. Laughter is the best way of announcing the friendly, playful mood.

Psychologie et métaphysique. J. P. DURAND.—As the new psychology tends to condemn metaphysics, positivistic materialism proposes to annihilate psychology. Both are wrong. There is a perma-

nent field for psychology, which is not even touched by other sciences, but psychology must admit its dependence upon metaphysics just as every other science does. The leaders in all sciences have employed metaphysics. A lengthy illustration of a metaphysical conception of the soul is given.

Perception et conception. HENRI ABIT.—There is a pure perception which is like the primitive tufa. The pure concept precedes this. This is the concept of space. The spatial idea is not the result of the perceptive process, but it is at the very bottom of it. The idea of space cannot be accounted for through perception, by making perception an active process.

De l'universalité du jugement esthétique. Professor VICTOR BASCH, Rennes.—Do universal æsthetic judgments exist? This brings up Kant's celebrated antinomy of taste. Every aesthetic judgment may be reduced to three principal factors: the directly sensible, *i. e.*, the sensual pleasure, which reduces itself upon final analysis to the consciousness of a maximum of stimulation with a minimum of fatigue; the formal factors, or the intellectual pleasure caused by unity in multiplicity; and the associative factors, which are essentially feelings couched in symbolic sympathy through which we may not only share intimately the life of beings separated from us by time, space, and unreality but may even confer our life upon inanimate objects. The directly sensible factor results from a sympathy between our nervous system and external stimuli; the formal factors result from a sympathy between the primordial law of our being and the forms of beings and things; and the associative factors arise from a sympathetic relation between past events in our life and the actual objects of our contemplation. The first two factors are relatively stable: the last is variable. The first two may be universally shared. Associative feelings, on the other hand, are essentially unstable, and their communicability depends upon an act of reconstructive sympathy which demands knowledge and a plasticity of imagination, of which half the world is incapable, and which is limited even among the élite to particular epochs and forms of art.

Psychologie de la timidité. Dr. J. P. HARTENBERG, Paris.—Bashfulness consists of fear and shame. It appears only in the presence of human beings, or with the idea of such presence. A description is given. Bashfulness may become morbid, and therapeutic measures are then justifiable.

Ueber die Aennlichkeit. Professor ANTON MARTY, Prague.—The speaker discussed the logical and the epistemological aspects of the principle of similarity.

Meine Auffassung der Willensfreiheit. WILHELM STERN, M.D. Berlin.—The speaker outlined the theory of freedom which is the groundwork of his treatise on positivistic ethics (Berlin, 1897). The doctrine is a subjective determinism, based upon empirical psychology.

The Value of Hypotheses in Psychology. Professor E. F. BUCHNER, New York.—Recent psychology has been engaged in gathering data—facts in and about consciousness. There is now a tendency to begin a generous discussion of the concepts derived from them. This praiseworthy attitude will lead to a careful analysis of the explanatory hypotheses by the scientists themselves, rather than by the metaphysician solely. The assumption of mental processes, of faculties *vs.* dynamic development, of psychical dispositions, of a simple relation between cerebral processes and conscious products—these hypotheses were discussed from a psychological point of view with reference to their adaptation to the phenomena of the science.

L'illusion du libre arbitre. SULLY PRUDHOMME, Paris.—"Whether free acts exist or do not exist, man has the illusion, at least, if not the real assurance, that, according to the witness of his consciousness, such acts do exist in the exercise of his will. This is an entirely empirical statement on my part, and I am entirely astonished at it; for is it not surprising, if everything in the universe is necessitated, that a mental state should be found from which to represent non-necessity, even if it is an illusion? From what combination of necessary factors can come such an image, whether true or false, of something which implies absolutely nothing of necessity and even represents its opposite? There is here certainly a problem to be solved. I can only call attention to it."

C. E. SEASHORE.

UNIVERSITY OF IOWA.

IMITATION.

The Theory of Imitation in Social Psychology. CHARLES A. ELLWOOD. American Journal of Sociology, VI. (6). Pp. 721–741.

This paper, which was read before the meeting of the Western Philosophical Association last January, is a criticism of the position that imitation is the sole method of individual development and social organization. The author advances four arguments against such a position. First, the selection of models for imitation, the fact that we do not imitate all the copies presented, implies the existence of other

instinctive factors beside imitation. Second, the theory makes too great a gulf between sub-human societies, organized on the basis of other instincts, and human society, which is supposed to be based on a single instinct, that of imitation. Third, it does not sufficiently recognize the importance of natural selection, in its psychical aspect, as a factor in human progress. And fourth, the theory 'rests upon no sufficient basis of ascertained facts.' It is, in short, too abstract, too far removed from the facts of life. "Both as M. Tarde and as Professor Baldwin conceive it, the social process is a process which might very well go on in a company of disembodied spirits—in a vacuum!"

There is no doubt that the author's general position is a safe one. One cannot read Tarde without becoming aware of the danger of supposing that the fact of repetition in society always indicates the process of imitation as its cause. But this pushing of the principle of imitation to its extreme limits is really an instance of the method that must be pursued in dealing with a new or a neglected factor. In order to find out what it will explain, we must proceed on the hypothesis that it will explain everything. It is well when the exploiter of a principle can also be its critic, but sometimes the constructive part of his work gains in enthusiasm and thoroughness if he leaves the limiting and paring-down process to others.

MARGARET FLOY WASHBURN.

CORNELL UNIVERSITY.

PSYCHICAL RESEARCH.

Proceedings of the Society for Psychical Research. Parts 36–40: Vol. XV. Feb., 1900, to Feb., 1901. Pp. 522.

Of these five parts, three (Nos. 37, 39 and 40) contain reports upon the meetings of the Society, Mr. Myers' presidential address and an address in memory of Professor Sidgwick. The articles calling for more extended notice are found chiefly in Parts 36 and 38, with one in 40.

Part 36 contains four leading articles. 'The Fire Walk' and 'Reflections on Mrs. Piper and Telepathy,' by Andrew Lang; 'Discussion of the Trance Phenomena of Mrs. Piper,' by Mrs. Sidgwick; 'On some Philosophic Assumptions in the Investigation of the Problem of a Future Life,' by F. C. S. Schiller.

Mr. Lang reports three recent, and some earlier, descriptions of the 'Fire Walk.' In the first of the three, Col. Gudgeon, British

resident at Raratonga, describes how he and three other Europeans walked barefoot across a circular area of red-hot stones 12 ft. in diameter, on Jan. 20, 1899. "I got across unscathed," says Col. Gudgeon, "and only one of the party was badly burned; and he, it is said, was spoken to, but, like Lot's wife, looked behind him—a thing against all rules. I can hardly give you my sensations, but I can say this—that I knew quite well I was walking on red-hot stones and could feel the heat, yet I was not burned. I felt something resembling slight electric shocks, both at the time and afterwards, but that is all." In the second case, Dr. T. M. Hocken, F.L.S., describes how at Mbenga, one of the Fiji Islands, he saw seven or eight natives walk across and around an oven of white-hot stones 25 or 30 ft. in diameter, leaving the circle at the point of entrance. Some of those who 'passed through the fire' were carefully examined both before and after their descent into the oven, but no clue to the cause of their immunity was found. The third case is reported by Col. Andrew Haggard, who witnessed the 'Fire Walk' at Tokio, April 9, 1899. Col. Haggard examined the feet of the performers afterwards, 'they were quite soft and not a trace of fire upon them.' Mr. Lang thinks that "every known physical or conjectural psychical condition of immunity fails to meet the case, and we are left wholly without an ascertained, or a good conjectural, reason why for the phenomena."

Both Mrs. Sidgwick and Mr. Lang attack the possession theory in the Piper case. Mrs. Sidgwick grants 'for the sake of argument that the evidence proves,' in this case, 'that knowledge is in some way derived from those who are dead,' but holds it does not prove that the communicating intelligence 'is any other than Mrs. Piper herself,' *i. e.*, a secondary personality of Mrs. P.'s, analogous to those studied by Janet and others. Mr. Lang will not go beyond the notion of 'telepathy *à trois*' in seeking an explanation for the phenomena, and would not be surprised 'if some normal explanation * * * were to be found' for them all.

Mr. Schiller's article is directed in the main against the assumption that between this life and that of the future world there can be no psychical continuity, an assumption which leads to the rejection of some evidence for the future life on the ground that it is 'not supernatural enough.' Mr. Schiller holds that the method of science requires the contrary assumption; the unknown must be construed in terms of the known until new experience gives a new 'known' where with to correct the initial assumption. In conclusion, he points out that from the position of an 'idealistic experientialism we may con-

ceive ourselves as passing through any number of worlds, separated from each other by (partial) discontinuities in our experience, each of which would be perfectly real while it lasted, and yet would have to be declared unreal from a higher and clearer point of view.'

Of the minor articles Professor Harlow Gale's 'Study in Spiritistic Hallucinations' is especially worthy of note. Professor Gale draws an instructive parallel between the case which he there reports and some of the Moses' phenomena.

The greater portion of Part 38 is devoted to Book II. of Professor W. F. Barrett's exhaustive monograph 'On the So-called Divining Rod,' of which Book I. appeared in Part 32, Vol. XIII. This second book alone covers 253 pages, of which about 100 are devoted to reports of new cases and experiments. In the remaining 150 odd pages are found much statistical and historical information and a study of the probable causes of the rod's motion. The whole study is illustrated by reproductions of photographs showing the scenes of the experiments, drawings showing some of the geological formations in which water has been found by dowsers, etc. Professor Barrett sums up his conclusions in pages 313–314. In brief, they are: The dowser meets with a degree of success which can not be due to chance and calls for some explanation. Whatever the source of his knowledge he does not consciously get it through the usual avenues of sense. It is usually presented to him as to others through the automatic motion of the rod; sometimes through certain sensations. Both of these are to be regarded as arising 'from a subconscious and involuntary suggestion impressed upon the mind of the dowser.' After making all possible allowance for suggestions unconsciously received through the ordinary channels, we are driven to the assumption that in some dowsers there exists 'some kind of transcendental perceptive power.'

Part 40 contains a brief preliminary report by Dr. Morton Prince upon 'The Development and Genealogy of the Misses Beauchamp,' a case of multiple personality. As Dr. Prince promises a more minute study of this most interesting case a detailed review of it is scarcely called for at present.

W. Romaine Newbold.

University of Pennsylvania.

NEW BOOKS.

Das sittliche Leben. H. SCHWARZ. Berlin, Reuther & Reichard. 1901. Pp. xi + 417.

L'année philosophique: 11e année, 1900. F. PILLON. Paris, F. Alcan. 1901. Pp. 316.

Psychologie de l'idiot et de l'imbécile. 2e éd., revue. PAUL SOLLIER. Paris, F. Alcan. 1901. Pp. iv + 236.

Pascal. AD. HATZFELD. (Grand philosophes series.) Paris, F. Alcan. 1901. Pp. xii + 291.

L'évolutionnisme en morale; Etude sur la philosophie de Herbert Spencer. JEAN HALLEUX. Paris, F. Alcan. 1901. Pp. 228.

Études de psychologie. J. J. VAN BIERVLIET. Ghent, A. Siffer; Paris, F. Alcan. 1901. Pp. 201.

Les timides et la timidité. DR. PAUL HARTENBERG. Paris, F. Alcan. 1901. Pp. xv + 264.

L'opinion et la foule. G. TARDE. Paris, F. Alcan. 1901. Pp. vii + 226.

Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie. H. Schiller und Th. Ziehen. Berlin, Reuther & Reichard. The following:

Die neueste Wendung im preussischen Schulstreite und das Gymnasium. I. F. HORNEMANN. IV, 2. 1901. Pp. 68.

Die Sprachstörungen geistig zurückgebliebener Kinder. ALB. LIEBMANN. IV, 3. 1901. Pp. 78.

Die Entwicklung der Pflanzenkenntnis beim Kinde und bei Völkern. WILHELM AMENT. IV, 4. 1901. Pp. 60.

The Circulation of the Nervous System. HERMAN GASSER, M.D. Plattville (Wis.), Journal Publishing Co. 1901. Pp. 156.

Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung. A. THUMB und K. MARBE. Leipzig, W. Engelmann. 1901. Pp. 87.

IVe Congrès international de Psychologie; Compte rendu des séances et texte des mémoires. (P. JANET, Secrétaire.) Paris, F. Alcan. 1901. Pp. 814.

Histoire et solution des problèmes métaphysiques. CHARLES RENOUVIER. Paris, Felix Alcan. 1901. Pp. 447.

Gustav Theodor Fechner. Rede zur Feier seines hundertjährigen Geburtstages. WILHELM WUNDT. Leipzig, W. Engelmann. 1901. Pp. 92.

The Play of Man. KARL GROOS. Trans. by ELIZABETH L. BALDWIN, with preface by J. MARK BALDWIN. New York, Appleton. 1901. Pp. ix + 412.

Notes on Child Study. EDWARD LEE THORNDIKE. (Columbia Univ. Contributions to Philosophy, Psychology and Education, Vol. 8, Nos. 3–4.) New York, Macmillan Co. 1901. Pp. 157.

Theologie und Metaphysik; Das Verhältnis der Theologie zur modernen Erkenntnistheorie und Psychologie. GEORG WOBBERMIN. Berlin, A. Duncker. 1901. Pp. xii + 289.

NOTES.

PROFESSOR KARL GROOS, of the University of Basel, has accepted a call to the University of Giessen as full professor.

PROFESSOR TH. RIBOT has retired from the chair of philosophy at the Collége de France (Paris) which he has occupied for many years.

AMONG the degrees bestowed at the recent anniversary of the University of Glasgow, psychology received recognition in the persons of Professor R. M. Wenley (Michigan) and Professor J. Mark Baldwin (Princeton); the degree of LL.D. was conferred upon each.

PROFESSOR CHARLES H. JUDD, recently of New York University, has been appointed professor of psychology and pedagogy at the University of Cincinnati. Professor Judd has been in charge of these departments at the summer school of the University this year.

WE note also the following appointments: Dr. M. F. Libby to the chair of philosophy at the University of Colorado, made vacant by the death of Professor Francis Kennedy. Dr. S. S. Colvin, Ph.D. (Strassburg), as assistant professor of psychology at the University of Illinois. Dr. Margaret K. Smith as professor of psychology at the State Normal School, New Paltz, New York.

MS. intended for publication in the PSYCHOLOGICAL REVIEW should be sent after October 1st to J. Mark Baldwin, Princeton, N. J.